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ON THE

NATURE, SIGNS, AND TREATMENT

OF

CHILD BED FEVERS;

IN A SERIES OF LETTERS

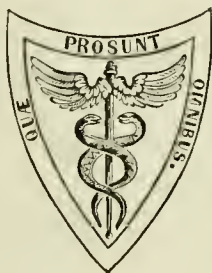
ADDRESSED TO THE STUDENTS OF HIS CLASS.

BY

CHARLES D. MEIGS, M.D.,

...

PROFESSOR OF MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN,
IN JEFFERSON MEDICAL COLLEGE, PHILADELPHIA, ETC. ETC.



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TO

R. LA ROCHE, M. D.

MY DEAR LA ROCHE:—

THE constant and loving friendship, that for more than twenty-five years, in uniting me with you in many interesting affairs—literary, professional, and others—has largely contributed to my happiness, makes me desirous of some suitable occasion to offer you a memorial of my grateful regard, and at the same time pay a tribute in acknowledgment of the eminent station you have acquired as one of the most learned and accomplished of American Physicians.

I should take far greater satisfaction in dedicating this volume to your name, if I could believe it quite worthy of your acceptance and approval; but, as it is probably the last formal contribution I shall have it in my power to make to our American Medical Literature, I will not allow the occasion to pass without offering it, and bespeaking for it, at the same time, your favorable consideration. I owe you so much, that I would to God I could bring you a better gift, for I consider that whatever of success I have had as a physician is, in a measure, due to your good and loving counsel, without which I should perhaps have never become a public teacher and a writer on Medicine, nor have been much employed as a practitioner. It was you who, many years ago, drove me from idleness to industry, and from presumptuous reliance on my own power of observation in Clinical Medicine to an acknowledgment of the advantages and necessity of studying the writings of other physicians. I am, therefore, deeply your debtor for many things in addition to that kindly regard that has been the pure source of many pleasures for me.

Perhaps you may even yet remember the occasion when, so many years since, as we were strolling of a summer day, under

the elms on the west side of Independence Square, I was boasting of the superior advantages of clinical observation as a means of learning to be a physician, and expressing my distrust of, as well as my disgust for medical books, which, of all things, I found the most wearying and unsatisfying; you came to a full stop, and, turning towards me, said: "Now, Meigs, you may depend upon it you are wrong in that, and I advise you to give up such opinions altogether, because you will never succeed as a physician unless you do read, and read a great deal on Medicine. No man ought to be so self-conceited as to suppose that, out of his own mere observations and reflections, he can, during a short term of existence, acquire what it has taken mankind twenty-five centuries to learn concerning the nature of diseases and their cure. Such a thing is impossible; and you will make an egregious mistake if you suffer yourself to believe that you can ever become a learned, useful, and successful physician, unless you should give yourself up to the reading of works on Medicine; which if you will do, you may succeed and do well, but cannot on any other terms." I well remember that you looked at me and spoke to me with the air of a young Gamaliel, and I, knowing that you but advised me to follow in your own good and true lead, listened attentively, and heard and was convinced.

I cannot pretend to say that, after so many years, I am now able to repeat exactly the words you addressed to me on the occasion, but I believe they were nearly those above written; certainly, I have recorded the very spirit of them, and even now remember how much I was shocked and humbled, though convicted, by your timely and friendly rebuke. Though it would not be proper for me to say more than that from that time I became a reader in Medicine, I consider it would be neither immodest in me to acknowledge that it is to you I am indebted for what habits of medical reading I have since practised, nor vainglorious to add that I have, in consequence, met with a fair degree of success as a professional man; because, I wish here to repeat that I am, in this measure, indebted to you for my present easy circumstances, for the privilege of being an American medical writer, and for an enviable station as Professor in a flourishing College of Medicine. Assuredly, but for your kind rebuke, I might have gone on to the present hour without learning any-

thing more than they taught me at college, and that routine Physic that even quacks and Thompsonians seem at last to acquire. Never did words spoken by one friend sink more deeply into the heart of his friend than yours into mine; and I can now do no more than thank you with all my heart while I dedicate this little fruit of your then goodness and sincerity to your honored name.

After this explanation, I must beg you to allow me to seize the occasion to say something about the book that I have laid before you, and call your attention to the Letter form in which I have written it; a form I was led to prefer by several motives. Among these motives was a sudden, off-hand promise I had made to the Students of my Class at the college. Having been much pressed for time, in the great number and variety of topics the discussion of which had led me to near the close of my curriculum of winter lectures, I had only two days left to devote to the consideration of childbed fevers, the history of which had conducted me to the last hour of the Term. Feeling how inadequate was that short time to the fulfilment of the important duty before me, I then engaged to finish my instructions as to childbed fevers, by writing and printing my thoughts concerning them in a series of Letters to those worthy gentlemen, and I have thus but executed a task undertaken out of a right motive. These Letters are, therefore, to be considered as addressed to my own pupils of that year; and, notwithstanding many of them are already become successfully established as professional men, I feel that I still have a right to speak to them as I used to do when we met in our lecture-room. It being to those gentlemen that I have written, it is for them to judge whether I have not used a language and manner very similar to that I employed in my public ministry, as their Professor of Midwifery, and the Diseases of Women and Children.

This book was never written for you, nor for men of your age and experience in Medicine: still, I trust there may be some things set forth in it, neither unpleasant to read, nor uninteresting in the details for many a medical brother who has not seen the topic treated so fully and methodically as I have here attempted to do.

I am well aware that some of our people do object to see medical subjects treated in this way, supposing that the Letter

form is not suited to the dignity of such topics; but as I could never, for the life of me, discern why a writer should not make use of the method of communicating his opinions that suits him best, I resolved to adopt this one because it gives me less trouble, and because I believe that the more direct, plain, and simple one's discourse is, the easier is it to be understood. In adopting this Letter form of speaking to the young gentlemen whom I have addressed, I seem to be still surrounded by them in my lecture-room or my library, and conversing with them *viva voce*. Moreover, we have so many medical writings, under the form of letters, that I am, in this, only following the example of my predecessors and betters, and so, take a warrant from them. '

A strong motive to make this book was also found in the fact that we have, in our American bookstores, only one really useful work on the special subject which is universally admitted to be of the very highest importance. I allude to the thin octavo volume published by Messrs. Haswell & Barrington, one of the series of our friend Dr. Bell's Medical Library, and which was a reprint of Gordon's work, with Hey's, and Armstrong and Lee's on puerperal fever, with an introduction furnished by myself, and containing my then views on the disease. That volume is certainly a very rich one, and would be so did it contain only Alexander Gordon's treatise on the epidemic at Aberdeen; for I conceive that no man can have any just notions of the treatment of childbed fever until he has read Gordon's book. But, since his day, so much light has been cast on the subject, that the time has arrived to present that subject to our American physicians as it is now; and this is what I have endeavored to do. It is for others to say whether I have done it in a useful manner.

The object of every publication ought to be to do some good, and I cannot but indulge the hope that the pains I have herein taken may have some useful results for physicians as well as the public; for I believe I have put down, in my Letters to my Students, the very best and most reliable opinions that I could collect out of all the many writers on childbed fevers that have come into my possession; a thing which, so far as I know, has not yet been done by any other author.

Having been exclusively concerned in medical affairs since 1809, and engaged in the Practice of Physic since 1813, I ought to be

in a way to be able to judge that there is no subject in the wide comprehension of medical clinics that requires a man to know more accurately what he is about than that of the diagnostication and conduct of childbed-fever cases; and if I should be found to have given, even some very small helps in so great a matter as that, it shall assuredly be admitted that I have done some good by this publication, and, if so, I may hope to avoid Fichte's condemnation.

Should you then approve of this labor of mine as to the probable tendency of it in helping the practitioner, I trust you will not object to the manner of it, solely because of its Letter form, but accept it for the good it may do, notwithstanding you might prefer to see me write in some other way. Indeed, I cannot imagine, nor ever could, that Medicine is a thing of that dignity and authority that it should suffer disparagement and loss because it is not treated of in a particular formal style of composition; because, the dignity of Medicine depends less on its rhubarb and its jalap, on its viscera and its vessels, its bones and their marrow, than on the moral conduct and intellectual attainments of those who study it as a science and practise it as an art before the people of the land: Medicine is Doctors, not physic. If I were about to write a deed of trust, or an indenture to convey an estate in land, I should endeavor to conform to the forms of such writings; but desiring to say to those young gentlemen what I know or believe on the subject of one of the most insidious, dangerous, and often intractable of disorders; one, the outbreak of which always carries terror with its invasion, pain with its progress, and often, unspeakable distress and woe as its consequence, I wished to speak to them just as I would do were I walking with them in the street, or conversing with them in a parlor, on a subject of the most urgent vital interest both to them and to society.

As to the style of these Letters, I am not a fitting judge, nor should I be allowed to judge were I the most fitting person in the world to do so; because they are no longer mine, but are become the property of whosoever may choose to possess them. There is, so far as has come to my knowledge, no style particularly appropriate to the discussion of questions in Physic; all that is wanted in it being, to tell the truth, as plainly and honestly as

possible. To do this, every man ought to follow his own bent, and write like himself, and not like everybody else; and if the public don't like it, the public will find the remedy is easy: they have only to laugh at it and let it alone, for a book will soon die that is neither amusing nor useful. Assuredly, as I thought, on that occasion, twenty-five years ago, our medical literature has little in it amusing; and yet, I see not why we physicians should ever wear sour faces merely because we are doctors, and ever fear to smile lest we should not seem sad enough for our melancholy vocations among the sick and the dying. If our writers would but mix a little more sugar with their physic, the young men would take it better; for it would then be like milk for babes, and they would find themselves seduced, instead of being compelled, as now, to read our tiresome tractates on subjects that have little in them of a nature to win by pleasing. Young men are not old enough to be as grave as the seven wise men of Greece, nor will they become so until they become oldish.

But, my dear friend, as to the style of this book, I may remind you that I never had, these forty years past, any time to study that part of writing; and I never shall make, as indeed I never did make any pretensions to the possession of writing-skill. I never sought for a word in my life; they jump into my mouth when I want to speak them, or they drop from my pen when I would indite them. But I don't think that any person can write well, except he write deliberately and with constant attention to the manner of his writing. If, therefore, I must either endeavor to write elegantly, or not at all, I must hold my peace and go down to my grave, after a long practical career, with the fruits of so much labor and anxiety of no value, except it might be deemed so in the limited circle of my patients, and by my personal friends and acquaintances. But, I do not think that a physician who has enjoyed a very large clinical practice for many years, ought to be content to die without, in some way, showing his sense of obligation to those who have sustained him all his life long, as well as to the profession that has admitted him in its ranks. Is it not true that both you and I have often mingled our regrets on seeing certain gentlemen depart from the scenes of their ministry, leaving no memorial of their experience or their thoughts for those they left behind to pursue a similar toilsome path, one in which acqui-

sitions are made with pain and difficulty, and amidst doubts and misapprehensions? I could not feel content to do so, and therefore, notwithstanding the perpetual interruptions and the fatigues to which I am still subject, I have written these Letters, with less regard, I confess it, to the manner than the matter of them. You are aware that I have already appeared before our brethren as a writer on different occasions.

I believe that the only American work on the subject of Obstetrics that was in existence, when I began to study Medicine, was the small duodecimo *Compendium of Midwifery* by the venerable Dr. Samuel Bard, of New York. We had, at that day, reprints of Denman's *Midwifery*, and the two volumes of Burns, edited by Dr. James, the London Practice, pirated from Dr. Clark, and a very much razeed copy of Baudelocque's two fine volumes. I apprehend that, at that period, few of our American practitioners possessed more obstetric works than those I have above named, and not many, all, even of these. Since that time, we have had the benefit of Dr. Dewees's contributions, and some few others of less note than his, among them my own. There is a ground, then, in this little progress, to hope that we shall gradually obtain an American medical literature on this branch, as well as in other branches of our science.

At the present time, we are abundantly supplied from abroad. I recently took the trouble to prepare a translation of M. Colombat's Treatise, and also one of the able writings on Midwifery, of the celebrated M. Velpeau. Besides these, I have printed my own work on *Midwifery*, and one on the *Diseases of Women*, which appear to have been well received, if I may judge from the demand for copies and new editions of them. My Essay on the *Acute and Chronic Diseases of the Cervix Uteri*, and a small Tract *On Certain of the Disorders of Young Children*, with the present *Letters on Child-bed Fevers*, comprise all that I have done in the way of medical writing, if I except many articles in the shape of journalistic Medicine. I presume that I shall stop with the present volume; and, indeed, I may say that I must soon stop altogether, for I am heartily tired of the life of a Physician, and earnestly desire before I die to have, if it might please God, some small time of preparation for the change that must soon come to both you and me. But you have much the advantage of me, because you are

younger, and have leisure to study, and had, indeed, a strong taste for hard study, when you showed me, what I wish you could show to every American medical student and physician, that to study is a duty imposed by conscience as well as self-interest. The work on Pneumonia you lately gave us, as evidence of this, is destined to be among the classics of Medicine; and if you will but carry out your long-cherished design of bequeathing to us the fruits of your thirty years of study and observation of yellow fever, you will have still a greater advantage over me, for it will place you among the first writers on Medicine of the present century; while my productions, alas! will be forgotten or neglected, except by some curious collector, desirous, perhaps, to see what were the thoughts of Obstetricians in the United States in the middle of the nineteenth century. How few of all the authors whom I have cited, in my examination of the doctrine of milk metastasis, are of any interest at the present day, beyond that of revealing to us the phases through which we have passed from their day to our own! Yet, among them, who doubts that La Motte and Mauriceau shall last as long as Horace or Cicero! I for one, cannot conceive that such literature as theirs is a perishable thing, but look on it as destined to accompany mankind down the long future lapses of time. Make haste, then, slowly, to lay your treatise on yellow fever before the world—for, as I sincerely love you, in the same sincerity, I rejoice in the prospect of your lasting fame. You have time to write useful things, and elegant; while I shall be but too happy if my American brethren, overlooking the want of elegance, should bestow on me the praise of making contributions useful to our art, promotive of our common progress, and so, beneficial to the public at large. It is only by repeated trials that the medical men on this side of the Atlantic, may hope to rival their brethren of the Old World. I admit that care should be taken to write as well as possible, by all those who sincerely wish to see established a respectable American medical literature; but a long time must probably elapse before we shall find writers like Coste, or Forbes, or Virey; because we are a working people, and cannot yet afford to sustain a purely literary class, like that of which those gentlemen are such pure specimens: we have not leisure, therefore, to consider so much the manner as the matter

of our communications. But the day of American literature will come, as M. De Tocqueville prophesies; and when it does arrive, the manner as well as the matter will, as he foreshows, be purely American. You have spoken very freely of our shortcomings, as medical *litterateurs*, on the eighth page of your introductory letter to the work on Pneumonia, but I think you ought to take encouragement to believe that your exhortations to improvement will be answered by a visible augmentation of the literary and scientific force of the American medical mind. For the present, I joyfully admit that our medical people are daily rising in intellectual vigor, and that the knowledge of the theory and practice of our art is not only quite equal to any just expectations to be entertained as to its usefulness, but that a love of study is far more wide-spread among us than it was forty years ago. Of a truth, we American physicians buy and read more medical books than any like number of our class beneath the moon. Hoping that these good prospects may soon become realities, I now bid you farewell, assuring you again of the affectionate attachment of

Your servant and friend,

C. D. MEIGS.

PHILADELPHIA, October, 1854,
324 Walnut Street.

CORRECTIONS.

At page 58, line 13 from bottom, *for attachés, read affichés.*

“ 58, “ 12 “ *for Richet, read Bichet.*

“ 290, “ 6 “ *for anaplastic, read aplastic.*

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PUERPERAL FEVER.

LETTER I.

MOTIVES FOR WRITING THESE LETTERS—GREAT DIVERSITY OF
OPINIONS ON THE DISEASE, AND WANT OF A BETTER UNDER-
STANDING OF ITS NATURE AND CURE.

TO THE MEMBERS OF MY CLASS OF 1850-51:—

GENTLEMEN:—

1. It is a very remarkable thing that, in the present age of the world, there should, among medical men, exist such great differences of opinion concerning the nature, causes, and treatment of one of the most familiar, and at the same time, most terrible and fatal of disorders; I mean the disease or diseases grouped together under the denomination of childbed fevers—puerperal fever, puerperal peritonitis, peritoneal fever, metro-peritonitis, fever of lying-in women, &c. &c.

2. One might suppose that time enough has elapsed, even since the revival of medical learning in the fifteenth century, and more especially since the surer establishment of our medical philosophy after Harvey's discovery of the circulation, and the great enlargement of the circle of science by Pecquet, Rudbeck, and Aselli's invention of the absorbents at a later period of the seventeenth century, to have enabled our people to come to some certain conclusions on these questions. Many thousands of cases of childbed fever have been observed—many hundreds of dissections have been made to investigate its *post-mortem* appearances—yet all these, with books, essays, pamphlets, and conversations without number, have failed to harmonize the opinions of our

✓ brethren; for, it cannot be denied that, at this time, there is no medical subject on which we are less inclined to agree, than on our rationale of the malady, or our views as to the best way of curing it. Many good authors have complained of this diversity of opinion among us, and Delaroche in particular, after showing how wide is the gap between different classes of physicians on this topic, says it is not only a subject of astonishment to those who have never studied medicine, but a source of sad reflection to ourselves. "One is, at times," says he (p. 44), "led to suppose that all our attempts to cure consist in giving some kind of a shock to the disordered machinery; that it matters but little what sort it may be, and that nature profits by the momentary shock to re-establish the equilibrium and mutual reactions of the organs."

3. Certainly, these differences of opinion among the doctors are very little creditable to them as a body of the learned; and, clearly showing that they have not availed themselves in a proper manner of all the means of knowledge within their reach, they at least evince that, with abundant sources of information on a special subject deeply interesting to the public, they have not exercised a due degree of diligence in searching after the truth, and so, prove us to be less learned and accurate, as a literary and scientific class, than we should be;—such short-comings are the proofs of our unworthiness as a scientific body. I have therefore earnestly desired that something might be done, with a view to harmonize our now discordant sentiments, since, in the event of the firm establishment of a clearly just doctrine, and right practice, as to childbed fevers, much good would accrue to society, whose servants we are, as well as to ourselves, who should expect to receive honor and reward only in proportion to our real merits and services to society. I confess that, while, on this subject, I do not consider the merits of the medical profession so considerable as they ought to be, I also believe they will never become so, until the time shall arrive when our whole corps shall be of one mind and opinion on these matters. That such a time is destined to arrive, there can be no shadow of doubt, for there is a truth in regard to the nature and treatment of our disorder, which mankind must and will find out; and whenever it shall come to be discovered, and made manifest, who is he that will be able to gainsay the truth!

4. The breaking out of an epidemic childbed fever, or even the occurrence of a single case, commonly excites a feeling of sharp interest and alarm, as far as ever the rumor of it extends: the public curiosity becomes speedily aroused; and among many physicians, whose opinions are taken, how wide is the diversity of their sentiments! The public curiosity thus left unappeased, the events, passing away, always leave a deep-seated conviction that, as the physicians disagree with each other, but little reliance is to be placed in them as a professional body, to this end; and so the disquietude remains, and we are disparaged, and our estimation is lessened. If the public do become greatly alarmed on these occasions, it is because they know that any one of our women, seized with childbed fever, is at once placed in a most perilous position—and we know it even better than they; and half our own distress and anxiety arises from this—that we are ourselves uncertain what we have to do, what to contend with, and by what principles to be guided?

5. The very name of childbed fever is a word of fear; to that degree, indeed, that smallpox, or yellow fever, when they attack an individual, or break out in a community, excite by the announcement of their onset scarcely greater apprehensions for the safety of the patients, or of society, than are aroused by the dreadful name just mentioned. Hence the subject is highly worthy of a Student's attention, and the more so from the diversity of the informations and numberless hypotheses in which he is to seek for the truth in relation to it. The works that have already been printed on the subject are more numerous, perhaps, than upon any single topic connected with obstetricy; and though it is true they contain an invaluable treasure of information, which the Student ought to feel himself bound by conscience to appropriate, and so furnish himself forth ready to combat against the dangers that threaten the persons committed to his watchful care, sagacity, and ability, yet the very number of those books renders the labor of reading them an almost endless task; a task which, I fear, is undertaken and performed by very few of us. Besides, upon consulting the books, the Student's mind is apt to become confused in the maze of conflicting views presented to him; so that when, at last, he finds himself, standing in the presence of the redoubtable enemy, called on to interpose his science

and skill to defend the woman from threatened destruction, he knows not where to strike, or how to interpose his shield; he gropes in thick darkness among his hypotheses, and what he does, being done without a clear understanding of what he should do, the victim is often lost through the physician's inefficiency.

6. Shall I then, while I complain that the books are already too many for us, shall I add to their number by contributing one more volume to the catalogue? Shall I add to the confusion I am so truly and so heartily ashamed of? No man ought to write a book unless he can set forth in it a new and useful thought, or place in a clearer view the thoughts of antecedent writers. This just sentiment I have derived from a writer who speaks upon the subject as follows:—

7. "Whatever may be the subject of a scientific work, the author of such a work must not conceive of knowledge in a mere historical fashion, and only as received from others; but he must for himself, have spiritually penetrated to the idea of knowledge on some one of its sides, and produce it in a self-creative, new, and hitherto unknown form. If he be but a link in the chain of historical tradition, and can do no more than hand down to others the knowledge which he himself has received, and only in the form in which it already exists in some work whence he has obtained it, then let him leave others in peace to draw from this fountain whence he also has drawn. What need is there of his officious intermeddling? To do over again that which has already been done, is to do nothing; and no man, who possesses common honesty and conscientiousness, will allow himself to indulge in such idleness. Can the age, then, furnish him with no occupation which is suited to his powers, that he must thus employ himself in doing what he ought not to do? It is not necessary that he should write an entirely new work on any part of knowledge, but only a *better* work than any hitherto existing. He who cannot do this, should absolutely not write. It is a crime, a want of honesty to do so, which, at the most, can only be excused by his thoughtlessness and utter want of any true understanding of the vocation he assumes."

8. These are the words of Prof. Fichte, which you may find at p. 126 of his Lectures on the Vocation of the Scholar; and they

are well worthy to be considered by every person who has the intention of writing a new book. I fully admit the justice of Prof. Fichte's condemnation of all those who, without due consideration, would add to the already heavy burden of our own peculiar literature, which, so abundant is it, has already become too vast for use, except by such as can give themselves up to a merely literary life. Yet, notwithstanding the philosopher's protest, which I have cited above, I feel impelled to write these Letters to you, partly in fulfilment of the promise I made at the close of my Course of Lectures—partly because I have felt obliged in this way to make up to you for the short-comings of my Public-instruction at the College, and, in a still stronger degree, because I think it would be a useful thing to set forth certain points that, though of the highest importance in our art, have not been adequately explained and enforced by those to whose labors we are indebted for the first suggestion and advancement of them. For example, in the use of venesection, as proposed by Dr. Gordon, while I look up to him as my real guide and teacher in this matter, I conceive that even he has in some respects failed to make his own wishes and sentiments clearly known. I should, I think, do a real service to my American medical brethren if I should be enabled to be a useful commentator on, and successful advocate of Dr. Alex. Gordon's great point of treatment.

9. Further, I am now sixty-two years of age, and first began to be conversant with medical affairs in the year 1809. I have had a very large and long experience in practice. I have written and published various essays, chapters, and lectures on this very subject. During many years I was connected with an establishment here for giving private lectures on Medicine, and it is now near fourteen years that I have had the honor to be one of the Professors in the Jefferson Medical College at Philadelphia. All these circumstances give me a warrant to speak, by this volume, on the subject of childbed fever; and as I conscientiously believe that another treatise is demanded, in the present state of our knowledge on this subject, I shall, therefore, keeping in view the warning and protest of the good German professor and philosopher, address myself to this task. If I have, indeed, attained to the full possession of the idea of childbed fever, both as to form and substance, my labor shall not prove to be vain;

not that I suppose myself to be in possession of knowledge in this matter that other men have not dreamed of—for I have no such vanity—I only hope, and humbly trust, that my laborious life has not been in vain devoted to studies connected with this department of Science, and earnestly desire, before I depart hence, to make one further acknowledgment of that debt I owe to those American brethren who have sustained me by their good opinions during so many years—all that I am, and have, being due to them; since I conceive that an American physician who is cast off and rejected by them is lost already.

10. In studying the subject of childbed fever, it will behove you to inquire what is its nature, its causes, tendencies, signs, and liability to modification and control by means of the medical art; to know whether it will be always one and the same—or whether it is not rather a group of affections, classed together under this one appellation. You should strive to attain to the possession of the idea, or spiritual image and form of the disease, as it is, not in nature, but in the intelligence; so as to create, as it were, a spiritual model, or *eidolon*, which you can denominate by this title—and which, once formed and perfected in your own understanding, might enable you to compare all the future possible events of your practice in this particular, with the true, absolute spiritual, intellectual standard, or idea of it. To attain to the possession of this idea, you should not rest content with learning what men of your own epoch have said on it—you must also have some clear notions of what was deemed and said in other times, and so, coming downwards from the beginning, make yourselves masters of its absolute identity, which consists not only in what is, but also in what was, and, to a certain extent, what shall be hereafter, as to childbed fever. I pray you not to make the mistake of regarding the Art and Science as already perfect—for, of a truth, Medicine is a thing always becoming, it has never yet become, and long ages must elapse ere it can reach such a stage of perfection. Hence, I propose in my next letter, to invite you to go back with me to an ancient date, that we may together for a moment contemplate this subject, as it was manifest, more than two thousand four hundred years ago.

LETTER II.

REFERENCE TO THE ANCIENTS.

11. I AM in the present communication to invite your attention to certain very ancient views of childbed fever, and I heartily wish it were in my power to set before you, in these communications, such a full and comprehensive prospect of the disorder, as might make you acquainted with it as it was in all past time, as well as show you clearly what it is to-day, and under all possible circumstances and forms of it. The former, however, I could not do, even if I possessed learning sufficient, without violating the plan of this work. I must, nevertheless, beg you to take notice, that as childbed fever consists of certain affections to which only lying-in women are liable, you are to conclude that it must have been familiarly known to the most ancient nations, and cannot but still occasionally break out in all lands; so that it is by no means a new form of disease discovered in modern times, but one coeval with the pains and risks of the puerperal state, in all ages of the world. We have no right to doubt that the women of ancient Thebes, Babylon, Athens, Rome, Byzantium, Alexandria, Venice, and Genoa, were as subject to its seizure as those of London or New York, and that many tens of thousands of them, managed by their Shipbrahs and Puahs, must have perished under its sporadic or epidemic forms. Hence the reflection arises that the Medical Sciences are of very slow growth, since in some thousands of years they have not grown up to maturity, even on this most trite and startling subject; it being certainly true, that we are still left to grope our way amidst the treasured stores of human experience, and are still seeking in vain for that full and positive knowledge that we so much need and so earnestly desire.

12. It is not necessary in citing the writings of Hippocrates, or other ancient authors, to settle, even were this possible, ques-

tions as to their personal identity. Such ancient productions, as Hippocrates's writings, are precious to us, rather because they come to us as reflections of thought from ages long past, than because they represent the ideas of any particular individual; for posterity always takes less interest in individuals, than in opinions, thought, rationales, dogmas, characteristic of times elapsed. It is sufficient for us to know that the books attributed to Hippocrates II. are among our most venerable records, and that they have for many centuries been the admiration of our class. Hippocrates II., the supposed author of the Books of *Epidemics*, lived at the same time with Socrates and Plato, and was therefore contemporary with many most illustrious and elegant scholars, in a day when admirable arts and important sciences sprung, as it were, perfect, upon the stage of the world, like Minerva fully armed.

13. Daniel Le Clerc, in *L'Histoire de la Médecine*, page 113, informs us that this prince of physicians was born in the island of Cos, in the first year of the eightieth Olympiad, about thirty years before the breaking out of the Peloponnesian war, A. M. 3005. The Peloponnesian war, according to Sir Isaac Newton's Chronology, p. 40, began 431 B. C., so that Hippocrates was born about 451 before the advent of our Saviour.

14. Those among you who should feel any desire to peruse a very severe and very learned history and criticism of the man and his writings, will be both instructed and amused by the curious volume of *Etudes Historiques et critiques, sur la Vie et la Doctrine d'Hippocrate, &c. &c.*, par M. S. Houdart, M. D. &c.; 2d edition, 1840.

15. Among the many exemplifications of diseases on which he treated in his writings, Hippocrates has left us accounts of several persons who were under his care in childbed fever, and they are so clearly stated that readers at all familiar with the disorder at the present day, readily recognize the pictographic excellence of his representations. In Book I. Sect. V., *De Morb. Mul.*, he already gives us a pretty clear account of childbed fever; but it is in the Books of *Epidemics* that we must look for his cases. Among others, here is one which gives you an account of the illness of Dromeades's wife, which is numbered Case XI. in the I.

Book of the *Epidemics*. Read it, and you shall not fail hereafter to meet with instances to convince you, not only of the fidelity of his relation, but show you that the disorder is one which was familiar in the most remote nations of antiquity:—

16. “The wife of Dromeades, recently delivered of a daughter, was doing as well as usual, when, on the second day after her delivery, she had a severe chill and a high fever. From the very first of this attack she complained of pain in the hypochondria, accompanied with nausea, rigors, anxiety, and sleeplessness, which continued for some days. The respiration was slow and sighing. On the day succeeding the chill, she had free healthy evacuations by stool; the urine was heavy, white, and troubled, like urine disturbed after long resting, but without sediment. The night was sleepless. On the third day, about noon, a violent chill, high fever, and urine as before; pain in the hypochondria; qualmishness; a bad night, without sleep; general sweat; coldness, promptly succeeded by a return of the heat. On the fourth day the hypochondriac pain was less: she had gravative pain in the head; lethargy; a few drops of blood from the nostrils; the tongue dry; thirst; urine thin and oily: a little sleep. Fifth day, thirst, nausea; same state of urine, no stool. About noon violent delirium, soon after which she became calm, and fell into a lethargic state. A slight coolness in the course of the night; sleep; delirium. Sixth day—in the morning a chill, followed by a prompt return of the heat. General sweat, cold extremities, delirium: slow, suspirious respiration; soon after which, spasms, beginning at the head, and which speedily terminated in death.”

17. Hippocrates, you see, speaks of pain in the hypochondria, by which you are not, I think, to understand literally the region of the body now so called, but the under parts of the abdomen, which is the usual seat of the pain in such cases as that he describes. Admitting this interpretation of his word hypochondria to be correct, then any person familiar with the incidents of a childbed fever will plainly perceive that the wife of Dromeades fell a victim to inflammation originating in the parts contained within the pelvis, and extension of that inflammation to parts above the plane of the superior strait, as well as into some of the larger veins or sinuses of the womb, for she manifestly exhibited

during her illness, the proofs that she had infected the mass of the blood with corpuscles of pus, that could only have been produced by the inflamed tissue-surface of a vein or veins. I take this to be evident by the hysteroidal state, or pyæmic-intoxication condition of the woman on and after the fourth day. No such delirium is to be looked for in the pure peritonitic forms of the malady.

18. Case X. of Book III. contains an account of one of the servants of Pantimedes. This woman being prematurely confined, was, on the first day seized with intense fever, and other symptoms of disease, of which she died on the seventh day; and doubtless with the same childbed fever we so often witness in the United States. As the details of the observation are quite meagre, I shall not quote them here.

19. The XIIth Case, Book III. contains the relation of another instance of the disorder. A woman, who in a laborious labor gave birth to a male child, was attacked with fever, thirst, nausea, and cardialgia, dry tongue, disorder of the bowels, marked by thin, small, liquid dejections, and insomnia. On the second day she had a slight chill, followed with smart fever. There was a moderate cool sweat about the head. On the third day, griping stools, that were copious, crude, and thin. The fourth day another chill, with general reaction and insomnia. The fifth day was bad; and the sixth was similar to the fifth: she had copious, liquid stools. On the seventh day the chill returned, with smart fever, considerable thirst, and violent agitation: towards evening there came on a universal cold perspiration, followed by cooling of the whole body, particularly the limbs, which could not be kept warm. At night another chill; and the extremities continued to be cold. She got no sleep, and was slightly delirious, with frequent lucid intervals. At noon, on the eighth day, she had febrile heat, thirst, great heaviness, and vomiting of small quantities of yellow bile. The night was bad, with insomnia. She passed the urine in great abundance, but involuntarily. On the ninth day, there was remission of the symptoms; heaviness in the evening, with vomiting of small portions of bile. On the tenth day, a violent chill in the exacerbation of the fever; obstinate insomnia. In the morning, urine very copious, without sediment; the extremities reco-

vered their warmth. On the eleventh day, vomiting of greenish bile, soon succeeded by severe rigor and a return of the coldness of the extremities. Towards evening, sweat, chills, and frequent vomiting. The night was bad. On the twelfth day, the vomiting increased, and she brought up a great quantity of dark fetid matter, which was followed by frequent hiccough and excessive thirst. On the thirteenth day, bleeding at the nose, which was succeeded by her dissolution. The stools were always frequent, and were accompanied with continual rigors. The patient was about 17 years of age.

20. Few medical men, upon examining the foregoing relation, I suppose, would fail to discover in it the proofs that this woman died with a childbed fever, and that the fever was nothing more nor less than an open manifestation of such constitutional irritation and nervous perversion as might naturally be expected to arise from an inflamed state of parts shortly before concerned in the acts of gestation and parturition. I conceive it is not required, in order to satisfy our curiosity in regard to the causes and course of the phenomena, to suppose that the woman was first affected by the causes of a fever—properly so called—and that the local inflammations and degradations were accidents supervening during the fever. It is far more consonant with a spirit of right reason, to construe the circumstances in an opposite way, and consider that a certain area or areas of phlogosis being first established within the pelvis, the febrile phenomena were awakened by that cause. Galen, in his second commentary on the third book of Hippocrates, cites this very case, and says that Hippocrates would have mentioned the fact, had any external cause led to the premature delivery of the servant of Pantimedes. The illustrious commentator remarks: "*Itaque ex morbo constat vitiosis collectis humoribus abortisse;*" that is—"It is, therefore, clear that she miscarried on account of a disorder arising from a collection of vitiated humors." Galen, in a long comment, argues to show that the woman's delivery prematurely, was due to the bad state of her health, which he deduces from Hippocrates's description of the fever—thirst, dry tongue, disordered bowels, &c. &c.—but he also expresses a suspicion that these affections might be the ready results of an active and distinctive phlegmasia; yet I conclude from his words that he knew as little what was going on in the case as his great prede-

cessor himself. In none of the cases is any memorandum to be found of the treatment, except, indeed, where the author says that the extremities could not be kept warm; whence I conclude that attempts must have been made, though vainly, to restore the temperature. Galen's comment on the case before given, that of Dromeades's wife, is too short to be of any interest.

21. Celsus, who speaks of the uterus and some of its morbid states, has left us nothing at all, worthy of citation, upon our particular topic.

Areteus was a Greek physician who flourished about Anno Domini 150, which is some ten years later than Galen, which leaves us to suppose he may have been a contemporary practitioner. In the whole of his eight very dull books, there does not appear the least notice of the disorders we are studying, so that, as far as concerns that particular interest, he might as well have never been born. The epoch of Areteus is a disputed point, however, for some persons would make him a physician during the reign of Augustus, while others carry him down to Adrian and Severus.

Oribasius was a voluminous writer who lived in Julian's age, and who accompanied that emperor in his expeditions, and is said to have assisted in establishing him on the throne. The 112th chapter of the 4th book might, *à priori*, be supposed likely to contain his views on those disorders that attack women in their confinement, for it is headed: "Ad vulvæ inflammationem, dolores, ulcerationes, mordicationes, pruritus sinus muliebris, ad eandem procidentem, et item ad ea omnia quæ ad conceptiones et fœtus pertinent." This entire chapter, however, consists in only thirty-seven lines of a double-column page. It has not even a reference to the disorders that follow childbirth. The 50th chapter, 9th book of the Synopses, "De uteri inflammatione," is utterly *jejune*, and even childish, except that he advises the woman to be bled at the arm on the third day, provided certain puerile methods indicated by him should fail of a cure.

Aetius, author of the *Tetrabiblon*, was of the fifth century. When these works of Aetius came out translated by Janus Cornarus, in 1541, they were highly applauded and eagerly sought after, but I presume few persons now look into them, except with the hope of catching a reflection of the medical opinions of that

remote age. Aetius wrote a great many chapters, and among them he treats, in cap. 38, tetrab. iv., sermo iv., de uteri inflammatione. In all this chapter on uterine inflammations, the only allusion to our topic consists in his mentioning abortions as sometimes being the cause of inflammation of the womb. It is a singular thing that Aetius, who, in tetrab. iv. sermo iv., devotes twenty-five chapters to midwifery, and another series of thirty-five chapters to diseases of women, should have said not a word on that great and important group of disorders that are treated of in these Letters, though, as compiler rather than author, he had ransacked all the works of his predecessors to make up his commonplace-book of a *Tetrabiblon*. It is probable, indeed, that what he did know was traditionally rather than empirically acquired. He confesses as much in his 14th chapter, when he says on *Parientibus facienda*: "Quæ præparare oportet, et quomodo secundum naturam parientibus consulendum sit, supervacuum fuerit scripto prodere, quam ex longo usu, non modo obstetrices, sed reliquæ quoque mulieres ista sciunt."

Alexander Trallianus, a Lydian Greek, came later than Oribasius and Aetius, but earlier than Paul of Aegina. The author of his article in *Biog. Med. of Dict. des Sc. Med.* assigns his epoch with that of the Emperor Justinian, and says that one of his four celebrated brothers, Anthemius the architect, was employed by Justinian in the construction of Sancta Sophia at Constantinople. Alexander of Tralles is looked upon by some writers as second only to Hippocrates. Still, whatever may be his merit as a writer on physic, he has none as a writer on our subject, for it was in vain that I searched his pages for a dot of light upon the opinions of his time concerning childbed fevers.

Paul Aeginetta, though in his treatise on inflammation of the womb whether connected with pregnancy and labor or in the non-gravid state, he shows that he had a very plain view of the fact that the constitutional accompaniments are due to it and to it alone, yet I shall not trouble you with a citation from his article, which you may consult at page 620 of the Sydenham Society's edition, Vol. I. Paul flourished during the seventh century, and, as he was a voluminous writer, and one supposed to be well acquainted with our Medical literature down to his own epoch, we may regard his silence as to any decided progress, as a sure sign

that nothing considerable had been effected during the eleven hundred years from Hippocrates to Paul.

22. Upon the decline of science accompanying the ruin of the Roman Empire, philosophy took up a temporary abode under the protection of the Caliphs, and it is certain that many able medical men issued from the schools and colleges established by those magnificent yet semi-barbarous sovereigns. Among those physicians, Johanna Ben Massoviah, a Syrian Christian, known in Europe as John Messue, was physician at the court of Haroun al Raschid, who died A. D. 801, and was a very learned man of that School; where also flourished at about the same date in the ninth century, the Bachtishuas, Gabriel and John. Messue has absolutely nothing worth quoting for you in his works, and I shall therefore content me with referring you to his "*Particula tertia sectionis primæ, continens summas quatuor—Summa prima, que est de cognitionibus meri*"—a chapter too stupid, I should think, to invite your attention. Nevertheless, it shows that nothing was yet done, though the doctors lived in clover, as we may judge from their great intimacy and familiarity with those dangerous and uncertain people, the Caliphs, whose whims and caprices sometimes rewarded us with purses stuffed with sequins, and sometimes, on the slightest occasion, amputated our heads with the cimeter, or choked us with the bowstring, or drove us into the outer darkness of disgrace and poverty. One of the most diligent and learned men of his day, was the great Arabian philosopher and physician, Abou Ali Houssain Ben Abdallah Ben-Sina Al scheikh Al Reïs, commonly known as Avicenna, who was born at Bokhara in the 370th of the Hegira, and died at Hamadan in 428. 370 of the Hegira corresponds to 980 A. D. The canon of Avicenna was, for our class, previous to the revival of learning in Europe, like a volume of the Scriptures for authoritativeness, until the writings of Hippocrates and Galen—of which they are, after all, but miserable compilations—allowed them to fall into merited contempt. Yet it is well to search the canons of Avicenna, to find what he knew about childbed fever. I have vainly examined the whole of the 21st Fen. of the III. Book, for an item of really useful thought, or the smallest mark of progress, and am left with the certainty that women at the court of Kouahrezm, where he was in high honor as a practitioner, either never applied

to him in their lying-in troubles, or, that if they did, he knew as little concerning the childbed fevers which must have prevailed, as the most ignorant and brutal Arab midwife. Yet, after all, it is easy to conclude that an Arabian physician could have little occasion for the study or practice of the Obstetric arts, since the Hareem is a sacred place among the Orientals, and it is far more likely that a Sultanna should be left to die in the hands of a wise woman, the traditional representatives of the midwives of the Scriptures, than expose herself to the manipulations of a male surgeon. I do not suppose then that medical men were called on to assist in labor, though the curious case of one of Haroun al Raschid's ladies, cured so singularly by Gabriel Bachtishua, shows that the doctors may have sometimes given advice outside of the Hareem. The story is told in the *Historia Dynastiarum* of Gregory Abulpharajius, translated by our colleague, Dr. Pockoke.

Averrhoes, who died in A. D. 1206, and Avenzoar, who lived to A. D. 1262, both Moorish physicians of Spain, have nothing upon our subject. Their works, like those just mentioned, show that, from Hippocrates to near the end of the thirteenth century, the illness of Dromeades's wife would be construed pretty much in the same way by all our brethren from Thasos down to Bagdat, Kouarezhm, and Seville.

23. Antonius Guaynerius, whose black letter quarto was printed at Lyons by Constantin Fradin in 1525, was an Italian physician, native of Liano, near Milan, who lived until 1440. He was much celebrated as a practitioner and writer, and was public professor, and, to judge from his *Opus Preclarum*, as it is entitled, was about as knowing a person in these matters as any man of his time. The 37th chapter on folio 165, "de regimine enixe," contains the sum of all he had to say concerning the diseases and accidents of lying-in women; and if you should adopt the opinion that childbed fever is, and always has been, the most considerable cause of the mortality of childbed, you will admit that, Guayneri, being a very fair representative of the learning of his time, there must have been very little of it, as far as regards Obstetrics, its diseases and accidents.

24. The same may be said with propriety of nearly the whole of the writers in Spach's collection, which, under the title of

Gynæciorum, sive de mulierum, tum communibus, tum gravidarum, parientium et puerperarum affectibus, et morbis, folio 1567, contains a considerable number of authors, who wrote on the diseases of women, and the disorders and circumstances of pregnancy and labor. The collection of Spach is valuable chiefly as a work in which a man can see how stupid the profession of Physic was up to a late period in the history of Letters. Spach's volume has transmitted to us the works on female complaints, of Felix Plater, Moschion, Cleopatra, Priscian, an anonymous writer, Trotula or Eros, Nicholas Roch, Bonacioli, Silvius, Ruff, Mercuriali, Montana, Trincavelli, Bottoni, Le Bon, Paré, Albucasis, Rousset, Caspar Bauhin, Cordæus, Martin Akakia, and Mercado. Among the best is the treatise of Mercatus. Louis de Mercado, known in our libraries as Ludovicus Mercatus, was a native of Valladolid, and was of fame so great as to be chosen Physician to Philip II., as well as to Philip III. He attained to the great age of 88, and died in 1599, about the time when William Harvey was a Student of medicine. As he died so old a man at the end of the century, you may set him down as eminent and active 35 years earlier, which will place you about 1564, when Spain was receiving the golden harvests of Mexico and Peru, just conquered, as we are now waiting our arrivals of two and a half millions a month from California. Mercado's work, *De communibus mulierum affectibus*, is printed in the closing part of Spach's *Gynæciorum*, and occupies its folio pages from 803 to the end of the volume at p. 1080.

At the 1070th p. he says: There is also a kind or condition of fevers that are peculiar to lying-in women, and proper to them only, and which is not even by chance to be ever observed in others, or at least very rarely; it depends upon suppression of the discharges, or on a diminished flow of the courses, and their becoming putrefied within the womb, or converted into a sort of suppuration. In such cases, fever cannot be avoided, as Hippocrates has taught us—"when pus is formed," &c. This is clearly shown by the uterine discharges, which, under such circumstances, issue forth with a strong fetid odor, and of a watery, sanious, or other unnatural character. Such fevers are similar to all other kinds arising from sources of putrefaction; from bubo or other unnatural tumor; from substances putrefying within

the womb, that the heart becomes heated, and that fever arises, is certain. That Hippocrates was well informed on these subjects, is evident from what he says in his first book, *de morbis muliebribus*, referring to the causes of fever in the lying-in woman by the words "when the discharges in a newly delivered woman do not go on well, as if the mouth of the womb were closed and its substance inflamed, the cavity of the organ is contracted with spasms after the escape of the foetus. If this should be the case the discharges cannot proceed." He adds the following as the prime causes, saying that if the effusion is suspended, fever is lighted up, with tympanitic distension of the belly. The woman cannot be touched even, without giving rise to pain all over the body, particularly if the abdomen be touched. The belly is much vexed with pains; there is also backache, loss of appetite, wakefulness, *punctura*, (*sic*), and after the 5th or 7th day the bowels are liable to become disordered with dark and offensive stools, and urine, *velut asinina*.

In England, so late as the reign of Henry VIII., there lived Dr. Thomas Raynalde, author of "*The Birth of Mankynde otherwyse named the woman's booke*," &c. This is a translation with modifications of Eucharius Rhodion's work *de Partu Hominis*, printed in 1535. My copy of Raynalde, 2d edition, is 1565. Now I would have you know, that women in King Henry's time had puerperal fever, as well as their predecessors and successors; and when attacked this is the way they were managed.

"Likewise do yf the woman have the ague after her labour, for that cometh of like cause, by retention of the floweres; and in the fever let her use to drinke water in the whiche is decocte barley beaten, or cider and barley together, or water in which is sodden TAMARINDS, or whay of mylke, and let her eate a cullis made of a eocke and sweete pomegranates, for these things do provoke the flowres, and mittigateth the immoderate heate, refreshing greatly the body, loosyng and opening suche thinges the whiche before were constricted and cluddered together. If the body after labour do swell and inflate, then let her drinke water in which is sodden eicer and cummin, beaten together."—Fol. lxxiii.

What do you think, young gentlemen, of our professional dignity and usefulness as conductors of childbed fevers in England

so short a time ago that four or five men's lives might possibly reach from Henry's age to our own? Certainly that was not an age remarkable for our progress.

25. Yet you see that even so lately as in the very close of the 16th century, this learned man had not gone one step beyond the date of Hippocrates, who flourished about B. C. 420; so that, since even Mercado and Raynalde himself had not advanced one step, we may say we all stood stock still on this matter for 2000 years. But as I am not proposing here to write a whole history of Medicine for you, I shall now cease to call your attention to the older volumes of our library. But it will be, I hope, for you, a pleasing reflection, that in our recent age we have made a most wonderful and gratifying progress in knowledge, though, as I have said before, we have not, even yet, found a common stand-point, whence we may all take the same view, and behold the circumstances as they really are, and all agree that we see them well and truly; for ours is a science that is in process of becoming and one that will yet become. We are still the victims of many a delusion in what is called the philosophy of Medicine, as well as in practice; we have still many barbarisms to be given up, for we doctors are a very traditional class of people. To learn that the barbarous customs of antiquity, in relation to many points in our art, were not laid aside even in the splendid age and court of Louis XIV., of which a singular proof is to be found in old Dionis's delightful little duodecimo, *Traité général des Accouchemens*, &c., Paris, 1724, open the book at page 327, and you shall read the following startling story:—

“There are certain teachers who insist that as soon as a woman is delivered, the skin of a black sheep, just torn from the body of the animal, must be laid over the patient's abdomen, under the idea that the warmth of such a skin restores and comforts the parts that have suffered. M. Clement applied one of them to Madame la Dauphine at her first confinement, but it was not repeated in her others on account of the inconveniences attending it. In fact, the precautions required are very troublesome, for a butcher must be had in readiness to skin the animal in an adjoining chamber, so that the skin can be had quite warm. The butcher who skinned the sheep for Madame la Dauphine's case, having taken it off and wrapped it in his apron, and having

brought it close to the bedside, the sheep, all bloody and naked, followed him to the bedside, which frightened all the ladies who were present at this spectacle."

26. Is not this a strange story, and could you believe that, though this bloody skin torn from a living animal that afterwards walked up to the bedside of Mary-Anne of Bavaria, at the birth of Louis, Duke of Burgundy, could not, under any circumstances, be of any aid to a sick person, it is still held by some in our most polished society as a most excellent cure; and that I, even I, have witnessed nearly such an application some fifty years ago, and have recently declined the proposal to apply one—a proposal that came from a high social quarter. It shows us how slowly we advance. But yet we have made, and are now making progress with great strides. I shall here close my communication, with assurances of that faithful gratitude with which I shall ever be your friend and servant,

C. D. M.

LETTER III.

INQUIRY INTO THE ORIGIN, PROGRESS, AND EXTENT OF THE MEDICAL DOCTRINES ON MILK-METASTASIS, OR WHAT THE FRENCH WRITERS CALLED ERREMENTS DU LAIT.

27. In this letter I propose to call your attention further to certain ancient opinions or notions, concerning childbed fever, whose former existence in our Class or Scholarship, though they be now supposed no longer to prevail, do, to a certain extent, still mould our views of diseases, and bias our methods of cure. I heartily wish I were capable of setting before you in these communications so complete a representation of the most ancient as well as modern notions, that my pages, like a bright Daguerreotype plate, might reflect the whole of the truth in even its most trivial and minute parts, and finally fix them there, so that, but to look upon them would be to have a conspectus of one entire medical cosmos—if I may be allowed to compare the small with the great. In educating yourselves as physicians, you ought to aim at the possession of a vast amount of knowledge of Medicine; for Medicine is an individuality or identity, which, to be thoroughly known, must be known not only as it is, but as it was; our successors in future ages, are they to whom is vouchsafed the knowledge of what it is destined to be. Our own mission is to know its past and its present states only; and we should be but imperfectly taught, if we stop short of the possible in these regards.

28. I have said, in another place, that words, written or spoken, are not always lost in silence as soon as the ordinary reverberations of them cease; but, on the contrary, they become a sort of material things, durable and indestructible in the minds of men; lingering among them for ages, and passing downwards with the

lapse of time to continually exert their mischievous or beneficial agency in human thought and action. How much more, when a generally received doctrine has become deeply impressed in the mind and heart of society, sealing itself, as it were, in the very constitution of the public intelligence and morals; marking a public opinion, and attending it even in its changes, yet modifying and biasing it even where its presence is no longer acknowledged, or even suspected!

29. Many such things have, through a traditional acceptance, become parts of the public faith, in both theory and practice; perverting facts, converting words into meanings foreign to their real functions, thus masking truth under a false disguise, hiding it with the mists of prejudice, and changing it into a lie.

30. In regard to childbed fever, which, as I have already shown you, was a disease frequently met with, though misinterpreted by the ancient, the mediæval, and early modern physicians, a particular interpretation of it grew up in Europe about the middle of the seventeenth century, and acquired so great a vogue, that one might venture to say it had an almost universal adoption, not only by our brethren, but by the common people; and being once established, it continued rigorously to control our medical opinions in this particular relation, until near the close of the eighteenth century.

31. Many of my readers will perhaps be disposed to contend, as certain writers have, that it died out then, and has been now for some seventy years completely expunged from the doctrines of the schools. But this is a point on which I shall feel constrained to differ with the majority, and believing that the long-cherished interpretation is, under a certain disguise, existing among us, lying concealed among the cloudy theories of many of us, I am about to develop for you the origin, nature, and extent of that curious and mischievous dogma of the metastases of the milk.

32. Thomas Willis, vide *Biog. Britan.*; sub voce, was born January 27, 1621-2, in Wiltshire, England; and after an illustrious medical career, leaving writings which have been handed down with veneration to our own class, died, Nov. 16, 1675. In 1655, Willis published his *Diatribæ Duo Medico-Philosophicæ, quarum prior agit de Fermentatione, sive de motu intestino particula-*

rum in quovis corpore; altera de Febribus, sive de motu earundem in sanguine animantium. His accessit Disputatio Epistolica de urinis. My copy is one of the 4th edition, 12mo. London, 1677.

33. Willis's chemical opinions respecting fermentation, led him to believe that not only life itself, but growth and nutrition, and death at last, are results of forces and actions of fermentable materials, whether they be blood, bile, chyle, or other substances, whose changes lay in us the foundations of fevers, dropsies, and other dangerous and mortal distempers. Hence you may perceive how he should conclude that the humors of the body, by their perversions and degradations, may come to play the most important part in the animal economy, whether in its healthy or its diseased states. There has never existed any class or sect of physicians to repudiate the opinion that the humors of the body, its fluids, are liable to become diseased, and so affect the health; but while certain sects have attributed greater, others have conceded less pathogenic power to the fluids. You would be in error then, to think that the humoral pathology as to childbed fevers, took its rise in the writings of Dr. Willis, although it is agreed, that to him, most particularly, attaches the repute of the promulgation of a dogma, which it is the main object of this letter to explain.

34. The sixteenth chapter, page 269, *de puerperarum febribus*, contains Willis's development of an idea concerning extraordinary particles contained in the blood. According to him, women, during their menstruating age, are subject to the periodical production, within the mass of their blood, of certain highly fermentable particles, which, if, contrary to the nature of the body, they happen to be retained within it, except the woman be pregnant, they give rise to a variety of disorders. In pregnancy, the menstrual material is not required to be cast out by the emunctories, for while ever the child remains in the womb, an alible substance or milk is continually being deposited in abundance about the uterine placenta, for the nourishment of the foetus. When the child has been expelled, the menstrual wants come to be supplied by the outflowing lochia at first, and subsequently, by the secretion of milk in the mammary glands. But if the discerning operations of the breasts should be lessened, or suspended, or wholly wanting, the material of the milk is then determined

towards and accumulated about the uterus, from whence it is discharged under the appearance of a whitish humor; and this is what is called pale or white lochia.

35. The author next proceeds to set forth his opinions in regard to the generation of the milk, whether that substance be generated in the breasts or in the womb; and also the metastasis of it from the one to the other; on the lochial discharges after the long suppression of the menstrua; on the state of the post-partum womb, and the influence of such state upon the other parts; and, finally, on the fevers of lying-in women, whether milk-fever or putrid fevers as they are called.

36. I should be well pleased to translate here, and so reproduce for our American medical literature, the whole of this celebrated physician's sixteenth chapter, but I do not feel assured that the taste of the majority of my readers would approve of so many citations in this book; and besides, the space allowed me does not admit of it. Indeed, I do not think you can read what I have already said of Willis's views, without becoming at once acquainted with the length and breadth of them. You have seen that, according to him, women are peculiar creatures in this, to wit, that they, at stated periods, separate in the mass of the blood certain fermenting particles, whose presence in excessive quantity does not fail to develop disease under forms diverse according to circumstances—disasters from which they are in general conserved by their menstrual evacuations, which statedly carry away these products forth of the body; by pregnancy, which consumes or appropriates them as the nutritive elements of the foetus; by milk or lacteous juices thrown off under the form of the lochia; and lastly, by the milk of the mammary glands, on which the newborn child depends for subsistence.

37. Such is Willis's hypothesis, which became, and still is, in many parts of Christendom, a dogma. In this sketch of it, you may readily enter into his views of the pathogeny of milk fever, retained or suppressed lochia, childbed fever, and all its diverse influences and consequences, in the human body. Nay, agreeing with him in his postulate as to the retained ferments of the menstrua, we may easily proceed with him to his final conclusions, and come to adopt in its fullest measures the milk

dogma, which, as I have said, acquired an immense influence upon the medical mind, and which still dominates over many a professional judgment, even in some who never heard of it before, nor even suspected its tyranny over their opinions.

38. I desire you should remember that Willis printed his little duodecimo in 1655, which was six years after the beheading of Charles I., and during the most brilliant period of the Protectorate, and during the administration of Cardinal Mazarin in France. It did not at once meet with the brilliant success as an especial hypothesis it afterwards found, particularly upon the continent. Indeed, it may be said to have never acquired the same authority in England as among other nations, though it was felt there, and still does influence British practitioners. If you will cross the Channel, you shall find that at Paris, there had flourished, some time earlier, an eminent medical person, Jacques Guillemeau, who was born in 1550, and died in the year 1613, leaving a reputation of the first mark, as an able practitioner and accomplished scholar, which he undeniably was. His is that ponderous folio, entitled *Les Œuvres de Chirurgie de Jacq. Guillemeau*, &c., &c., Rouen, 1649. He was a pupil of Ambrose Paré, and was a copious and pleasing writer, and an earnest student. Occupying as he did an eminent public station, you would naturally look among his pages for any clear traces of this milk dogma, or *lait repandu* hypothesis, if it was already in existence, and you would confidently expect to find them there, if anywhere in the medical literature of his era. Well, then, you shall in vain turn all the pages of Guillemeau, for any signs of the appearance of the dogma, whence you may safely conclude its dawn had not yet appeared to the principal medical men, about the close of Louis XIII.'s reign, and the days of Cardinal Richelieu.

39. You may now turn to a volume entitled, *Trois Livres appartenans aux Infirmitéz et Maladies des femmes. Priz du Latin de M. Jean Liebaut, Docteur Médecin, à Paris, et faits François. Rouen, 1609, 8vo. pp. 923.* The original Latin edition appeared, I believe, as *De Sanitate Fœcunditate et Morbis mulierum*, Paris, 1582, 8vo., and was translated into French in 1582. My copy is 1609, 8vo. Liebaut, according to the authors of the *Diet. de Biog. Méd.*, appears to have practised with much success at Paris,

where he died in 1596. This writer, in his 33d chapter, says: "The milk, therefore, is the benigu excrement of the breasts, ordained by nature for the nourishment of the child, whether within or without the mother's womb;" and at p. 777: "The menstrual blood, therefore, is not made aliment in the breasts, but altogether (*tout entier*) just as it is, and in just such quantity as it is when received by the breasts, it is converted and transmuted by the breasts in nature of milk, without any portion of it, no matter how small, being attracted by the breasts for their own nourishment, and from which the milk acquires its form, its nature, and its whiteness." At p. 325-7, he argues in favor of the idea that the menstrual blood is not an impure substance, but only the excess provided by nature above the ordinary sanguine mass; and hence, you perceive that in the time of Liebaut the notion of a *lait repandu*, or the milk dogma was unknown, for certainly as he was in much esteem at Paris, where he had a large business and numerous professional connections, he could not but have become acquainted with the idea, had it as yet arisen. Now this writer died in 1596.

40. I may be excused from citing the opinions of a woman, Louise Bourgeois, on the grounds that she was a very learned person, much acquainted with, and greatly esteemed by the principal medical men about the court of Henry IV., of France. We have the spirited and naive memoirs of her own life and proceedings as a Paris *sage-femme*, and particularly her relation of the several confinements of the Queen, Mary de Medicis, whom she attended in labor on the 7th September, 1601, 22d November, 1602, February 10th, 1606, April 16th, 1607, April 7th, 1608, and November 26th, 1609. To take her own account of these matters, she was in great estimation by La Rivière, who was Henry's physician, with Du Laurens and others, which afforded good opportunities for her to know of this great doctrine, if it had then already been bruited abroad. Louise gives us all her opinions in chap. 23, whence it is clear she never had the least idea of the so-called milk-infarctus, though, from a passage in Mauriceau, Du Laurens seems to have been somewhat infected with it.

41. As I have mentioned the name of La Rivière, I beg you to take up *Lazari Riverii, opera omnia*, &c., Fol. Frankfort, 1669.

At Book 15, chap. 24 of the *Pract. of Physic*, you will find whether he had heard of the milk dogma, or no. It is proper to say he was born 1589, and departed this life in 1655—the year when Willis's book *de febris* came forth from the press. Riverius contains not a word in that way; and, of course, as he was a most eminent physician, he had not heard it broached.

42. Among the medical classics of France, and, indeed, of the end of the 17th and beginning of the 18th century, there is no higher name than that of François Mauriceau. It is said of him by Dr. Begin, in his notice *Dict. de Biog. Méd.*, that he may be regarded as “the first French surgeon whose writings bear the impress of a true accoucheur;” that he marked out the path that was afterwards followed with such brilliant renown by Viardel, Peu, Portal, Deventer, Lamotte, and latterly by Smellie, Lauvergeat, Levret, and Baudelocque. This admirable colleague of ours was born about the middle of the 17th century, and lived until the year 1709, when he died on the 17th day of October. He died six years earlier than his master, Louis XIV. Mauriceau was engaged in practice until the close of his life, and among the most elevated classes of society.

43. There is a delectable story told by Bichet, author of *Obs. sur l'Art. des Acc.*, &c., 12mo. 1758, who describes at p. 85, the labor of Madame la Marquise de Clermont, daughter of Mons. le Marquis d'O, Lieutenant-General of the marine, who laid eight days in labor at Versailles. It would seem that the whole court was thrown into what the modern theatrical attachés call a wonderful excitement about it. Dionis was called in, and Richet says that “le célèbre Monsieur Mauriceau” was also present, and gave what appears to me to be a very shrewd opinion on the case of the sick Marchioness. This was in 1708, only a year before the decease of the good man.

44. Mauriceau was an elegant scholar, and being a leading man in the highest literary circles, he would not have escaped the infection of the *milk*, had it already risen to the height of his own level. At p. 412, tom. i. he says: “Those who, when they discover a sort of whitish lochial discharge, believe that the color is due to the milk of the mammary glands, flowing out from the womb, found their opinion upon the circumstance that, generally

speaking, the milk ceases to be formed by the breasts in proportion as these whitish lochia continue to flow; and they say further, that it is evidently milk, as every one may conclude from its consistency and color; but if they understood anatomy well, they would know that there is no vessel of communication between the mamma and the womb; unless, indeed, they should imagine the transfer to be made by a supposititious anastomosis of the mammary with the epigastric vein, a thing that cannot be, since neither the one nor the other of those veins goes either to the mamma or to the uterus."

45. He proceeds afterwards to show that milk cannot by any process be carried into the uterine vessels, and so escape from their orifices as white lochia. On this subject he reasons as well as any modern, in his luminous 413th page, closing his argument with a citation of Aristotle, B. 7, C. 2d, Hist. Animal. "*Natura enim ita fert, ne humor loeis pluribus, simul erumpere soleat.*"

46. Even in his chapter on phlegmasia dolens or erural phlebitis, one which of all others he might be expected to devote to what was, shortly after his time, denominated "*Dépôt laiteux*," milk deposit, lait repandu, and milk-leg, as it is vulgarly named to the present day, he does not even hint at the dogma we are investigating. Now you may consider that Mauriceau must have been near the head of his profession at Paris, from 1670 to the first years of the 18th century, and as he barely sneers as you see at the doctrine, which had not yet got up to his level, you may conclude that Willis's hypothesis had not fairly got across the Channel, and become established on the Continent in fifty years after its promulgation at Oxford in England.

47. We have the privilege to claim another most worthy colleague in our profession, that good honest man, Sieur de la Motte, sworn surgeon and accoucheur at Vallognes in Normandy, author of that famous *Traité Complet des Accouchements naturels, non naturels, et contre nature*, &c. &c., Paris, 4to. 1721. An honester or more sagacious practitioner has not lived since his day. He had an excellent bringing up, in the wards of the Hôtel Dieu, at Paris, and practised with much success and fame during many years; his volume is *aere perennior*.

48. Neither in his Chapter 6, Book 5, devoted to the history of

the lochia, nor in Chapter 7, Book 5, on inflammation of the womb, does he allude to lait repandu. But, you should note that la Motte was born in 1655, the year of Willis's publication, and lived until June 1737, and moreover, that he was a diligent student, and ripe scholar, whom nothing escaped. Then you must infer that the dogma had not reached him when he wrote, which may have been about the year 1708 to 1710, since the printed approbation of the surgeons of Vallognes, MM. Fromont, des Rosiers, and Hanouel, bears date April 16, 1712.

49. One of the most illustrious men of modern times was Frederick Hoffman, professor of the Frederick's Academy at Halle. His birth took place on the 19th of February, 1660, at Halle, and he died in the same city, October 4, 1742, being eighty-six years old. Hoffman's distinguished position as teacher, as writer, and as correspondent, would have made him familiar with the doctrine we are examining, and doubtless we should have had some marks of his opinion concerning it had it reached him, and been deemed worthy of his respect. The second volume, *opera omnia*, contains, beginning at p. 156, his 10th chapter, sect. ii. de inflammatione et febre uterina, both the 4th and 5th sections of which contain his views as to the causes of childbed fever under the aforementioned title, but there is nowhere to be found any elucidation or acknowledgment of this doctrine, even when he speaks of milk fever, which, occurring generally about the third day, he supposes may, when coinciding with a suppression of the lochia, give rise to those states of obstruction in the circulation that should result in his uterine inflammatory fever. Volume I. contains, at p. 80, his 14th chapter, sect. ii. book i. De Secretione Lactis. The whole article is free from any trace of the doctrine of lait repandu.

50. Nicholas Puzos was born at Paris, 1686, and died in 1753. I do not know a better, braver, or more skilful physician than Puzos. A learned and judicious man, he enjoyed an immense public confidence. It were indeed to be wished that our profession might be graced, by now owning many such persons as M. Puzos. I cannot peruse his pages but I am prompted to regard him as one of our most accurate and solid observers, and his *Traité des Acc. &c. &c.*, Paris, 1759, 4to. as one of the most pleas-

ing, instructive volumes in the medical library. It appears to me that he may have been in his most flourishing state about 1730-40. He is certainly to be regarded as one of the chiefest advocates and propagators of the doctrine concerning *lait repandu*, *Dépôt laiteux*, milk infarctus, and all the hypothetical consequences of Willis's notion.

51. I might select Puzos therefore as the expounder of the dogma, and confine myself to citations from his writings; but I shall prefer to go further, and to another and later fountain.

52. Puzos, however, opens at p. 218, Chapter 21, with these words, that serve to set forth a principle which, were it true in fact, might serve as its sufficient explanation: "Milk is undeniably the first, and the only nutriment the child makes use of in the first stages of its formation; and as soon as a woman becomes pregnant, she acquires the faculty of producing milk." After some remarks upon the failure, before his time, to present or explain a rationale of this operation, he proceeds to say that, "what the physiologists have most clearly taught upon this subject is, that milk is nothing but a species of chyle, which, in confusedly circulating along with the blood in the vessels, becomes charged with certain properties of the blood, and so ceases to be pure chyle." He explains that there are no separate channels, for the circulation of this milky humor, which, whenever the uterus is cut open, always appears as blood, as it does also when the placenta becomes detached. He insists that it accumulates in the bloodvessels of the womb for the nutrition of the foetus, and he uses these words: "It is also known that at the commencement as well as during the whole progress of pregnancy, the milk is determined in abundance towards the womb; penetrates into its interior to escape thence, subsequently, always mixed with the blood, in order to insinuate itself into the venous roots of the placenta, from whence it passes along the umbilical vein, which distributes it in the whole body of the child."

"When a pregnant woman is bled, the blood always assumes a whitish appearance on the surface, and there is no doubt that this whitish matter is milk, which, being the lighter portion, rises above the fibrous portion." 219.

53. Puzos assures us that its appearance is not evidence of bad

blood, and he asks whether the urine of pregnant women is not usually thick and whitish. Evidence of the same force he thinks is to be found in the milky effusions that we occasionally observe in the cavities and in the cellular tela, commonly called Dépôt lacteux, or milk deposits, &c.

54. "As soon as a woman is confined," says he, "the milk which had previously been regularly determined to the parts where it was continually being absorbed, of necessity changes its course to flow towards those parts from which it might most readily escape. Finding almost no further issue at the womb, it was proper there should be provided a sort of reservoirs, in which it might be detained for a certain space, and then eliminated."

55. These reservoirs are in the breasts, and Puzos remarks that, "but for this wise provision, the milk must be tumultuously thrown upon the different parts, and give rise to the same disorders that are often caused, when, through imprudence or any unfavorable disposition, it takes false routes and deposits itself among the tissues, which cannot free themselves from it." 220.

56. The above may serve well enough to show you how good old Puzos was accustomed to reason upon these questions, and indeed the general character of medical thinking upon them. It is not therefore required that I should accompany him in a further progress through all the possible consequences of a deviation or misplacement of this milk in the pregnant woman's blood. My design in quoting him at all, was, mainly, to show how far the doctrine had extended at the most flourishing period of his fame and usefulness. You will promptly conceive that a woman, attacked either in her pregnancy or lying in, with any violent disease, must be accused of some fault in regard to the milky element in her blood, and that, if she should die with apoplexy, or metritis, or peritonitis, or phlebitis, or no matter what, it is always, and under all circumstances, to some milk infarctus, or deviation and dissipation of it into unfit parts and channels, that the result is to be charged. Antoine Petit, a celebrated man of his epoch, *Traité des Mal. des Femmes accouchés*, &c. &c., p. 182, tells us that, "nothing is so common at Paris as milk-disease, that is to say, diseases arising from some vitiation, arrest, deposit, or depravation of the milk, from a suppression of the second-

ary and tertiary lochia. These disorders vary infinitely, according as the deposit takes place in the head, the chest, the belly, the extremities, or the skin."

57. Notwithstanding the surprise with which I find so powerful an understanding in this manner misled upon plain questions in physiology and pathogeny, I am still ready to confess my admiration for the man, and to warmly urge you in particular to study his great paper on *Dépôts laiteux*, which contains examples of practice altogether as admirable as the great practice of Alex. Gordon himself. I shall not follow Puzos any further, considering myself acquitted of obligations to you as to him, at least for the present.

58. The *Nouvelles Recherches sur la Fièvre Puerperale*, par M. Doublet, Médecin de la Faculté de Paris, et de la Société Royale de Médecine, was published 12mo. at Paris, 1793.

59. I might perhaps select this volume, as containing a most vehement defence and exposition of the milk dogma, a defence and exposition carried on with elaborate research and argument, as well as illustration—for the author appears to have examined almost everything ancient or modern, that might enforce or confirm his notions as to the universal derangements proceeding from error loci of the lactcal secretion. But, as I cannot without impatience, notice the violent twisting of the truth, and the violent perversions of his judgment, everywhere displayed, I shall pass by it with this slight notice, yet feeling that I shall not have fully answered the demand of this inquiry, unless I could persuade you to look with me into the "*Cours Elémentaires des Maladies des Femmes, ou une nouvelle méthode pour étudier et pour classer les maladies de ce sexe*. Par Marie Joachim Vigarous, Prof. à l'école de Médecine de Montpellier—Paris, 1801," 2 vols. 8vo., for Vigarous is the best defender and illustrator of the doctrine.

60. Any person who should desire to learn, in their fullest extent, the doctrines of the Milk school in Obstetricy, may be assured he shall find them set forth and vindicated in this work. Vigarous gives one hundred and eleven pages to the discussion, from 376 to 487, vol. ii., and they contain all one could desire of explanation. But let us hasten to see what Vigarous has left us, and respectfully hearken to the voice of the dead author, who

speaks to us in his pages, though his frame is now dust on the shores of the Mediterranean. An illustrious dead physician is still far worthier than many an incompetent living doctor. I would liefer live after I am dead than die daily all my life long.

61. At p. 376 he observes: "I have already said, that towards the close of gestation there is formed a great quantity of lymphatic juices, that are habitually determined towards the womb for the nutrition of the foetus; that subsequently to the birth, they continue to be evolved in abundance, and then change their direction, and convey to the breasts the materials those glands are destined to eliminate for the nutriment of the child."

62. The change of determination is not, in general, effected without a degree of fever, of little moment, and which is vulgarly known as milk fever. This, then, is the gist of the dogma; and, provided your studies have been of a kind to allow you to believe on the one hand, that blood is blood and something else besides—a mixture of blood and milk, you may set yourself up as a sufficiently good humoralist; but if, on the other hand, you are really to regard the sanguine mass as a specific thing, as much so as any particular bird, plant, fish, or worm; that it consists of fibrin 3, albumen 80, corpuscles 127, and water 790, I do not discern how you can be in danger of falling into the deep slough of the humoral pathology. On the contrary, I indulge the hope that a clear understanding of the blood's crisis, or constitution, may keep you harmless as to other poisonings of that vital fluid, whose least qualitative and quantitative modifications are found to exert so great an influence on man's vital state. How, indeed, should it be possible for you to fall into the errors of the humoral pathology, after you have once become satisfied that the blood is a mixture of certain solid constituents, 210, with pure water its diluent, 790; that the water of the blood is not evolved by the vital forces, but only absorbed; whereas the solid constituent, 210, is created, or at least transformed and evolved, by the powers of the solid living parts?

63. There are many various states of the bodily health that coincide necessarily with any alteration in the ratio of all these constituents, but there is not one of them that can convert the blood into milk as it flows in the bloodvessels, milk being a

production of the mammary gland, and not a direct result of the Hæmatosis.

64. Nevertheless, it is well for the Student to hear M. Vigarous, and I now beg you to observe that he quotes, approvingly, Battisti's thesis on the diseases of women, to convince that milk fever is of two kinds, one *ephemera lactea* and the other *febris lactea*, which arises "when a woman's breast, being already over full of milk, continues to receive an additional quantity, whereupon a portion of the excess is drawn into the torrent of the humors, causing symptoms of a continued remitting fever with quotidian type, ushered in by chills, and followed by a hot stage, and afterwards by a sweat of an acrid and peculiar odor, which resolves the paroxysm." 377.

65. Though this fever generally ends in a sweat, as above, "it may end by a spontaneous or artificial evacuation of milk from the breasts; by lochia, in greater or less abundance; by urinary discharges; by critical evacuations by stool; by a miliary eruption; by a metastasis to different parts, frequently attended with serious, and sometimes mortal consequences." 277. A milk fever is a thing of a purely nervous nature, and when all goes on well and properly, the case is one of *ephemera lactea*. It is a natural incident in what the French Doctors call *la revolution du lait*. But when it does not proceed in order then it becomes *febris lactea*. 378.

66. Vigarous might have stated an infinite number of divisions and classifications of the accidents and deviations of the milk, from Mons. Goubelly's two volumes of *Connoissances necessaires sur la Grossesse, sur les Maladies laiteuses, et sur la cessation des Règles, &c.*, par M. Cl. And. Goubelly, &c., &c., &c., Paris, 1785, for Goubelly practised and taught in his professorship, and wrote when the dogma was at its culmination. You would be amused to see to what a variety of applications it served in Goubelly's hands, as well indeed as in everybody else's. But Vigarous disapproved both of making three distinct species of milk fever, and also of absolutely confounding them together as one, though he admits their "successive affiliation." He considers it probable that the fever that follows upon a lying-in, has its source in milk fever, which affords occasion why different morbid causes should make

their power be felt; and he also supposes that the puerperal fever which attacks nurses, and women not suckling, and those fevers that arise after the termination of a milk fever, are due to a nervous spasmodic affection, analogous to that which precedes the milk fever. 380. "But it is certain that the puerperal fever is not a special fever, or *sui generis*, since it varies in different individuals; with the constitution of the year; and under divers causes that may complicate it or originate it." 380.

67. From page 388, I select for you the following curious thoughts of Vigarous, and first ask you to conceive, if you can, the process by which an intelligent reason can be so hoodwinked and betrayed, unless it be by the force of educational and scholastic prejudice, which, it appears, can compel men to go traditionally wrong. This physician had witnessed the dissections of many bodies of persons, dead with childbed fevers, and must have seen with his own natural eyes, the pus, the serum, the coagula of albumen, the adhesions and bridges that are left there by the inflammation, yet the scholastic eye of Vigarous interpreted all those products as milk, for he was the slave of the milk dogma, as were all the disciples of that whole school; and what is still more surprising, the ghostly form of this imaginary evil ens, still walks among the modern schools, mysteriously influencing the teachings and practice which, by this time, one would think, should have come to be based, not on belief, but on knowledge, not on *πίστις*, but on *γνῶσις*. These are the words:—

68. "The metastatic congestion of the lymphatic and milky juices, constitutes the natural cause of the malady (*childbed fever*); this is proved, not only by the effects of the puerperal fever in women dying with the disease, but also by the critical phenomena presented by those who recover from it." 388.

69. "We find in the abdomen a collection of *purulent* matter, which cannot be alone the product of an inflammation of the bowels and epiploon, because, as Sellé remarks, that inflammation is never in a direct ratio to the mass of purulent matter discovered. Ofttimes, the inflammation noticed is of small moment, but the quantity of pus great; at other times the amount of inflammation is considerable, that of the pus small." He ends the paragraph by informing us that, while inflammation does form one of the

complications, it also becomes one of the causes of milk-metastasis by exciting spasm, &c. 388.

70. Page 400 opens with observations on "Bilious gastric puerperal fever," which attacks in the summer and during the reign of a bilious constitution of the season. At 413 we have the "Putrid bilious puerperal fever;" at 438 the "Puerperal fever complicated with phlogistic affection;" at 450 "Sporadic puerperal fever from a nervous cause; taking cold, &c.;" and lastly, at 467, he treats of the different terminations of puerperal fever, where I shall cite him as follows:—

71. "We have now reviewed most of the forms of the puerperal fever, and have seen that the milky matter which plays the principal part in it, was set in motion by different causes, whether humoral or nervous. We have learned to combat these causes by all the means belonging to the art of curing, but vainly should we have labored to destroy the disease while the milky matter was left fixed upon the lower belly, and particularly on the bowels and chyle-duets. It becomes certainly mortal, when nothing can change its determination and open for it a new route towards the skin, for then it corrupts and gangrenes the vessels and all the viscera of the abdomen. We have seen that the whole science of the treatment consists in deviating the milky humor, while it is still movable, and in preventing it from fixing in the abdomen."

72. Few modern physicians have any idea of the extent to which this milk dogma prevailed over us from its uprising, about the beginning of the century, until near its close, say nearly one hundred years; nor do they know or admit, that to the present day it biases many opinions, and modifies, to a great extent, the practice of our art, under a changed form, it is true, yet still itself; so that one might say of it, *mutato nomine de te fabula narratur*. See what I have said, in sec. 186, on vitiated blood.

Professor Jörg, of Leipsig, was born in the year 1780, and of course could not rise into eminence earlier than the close of the first ten years of the present century. He seems to have acquired a great reputation as a practitioner, as well as a Medical writer; yet I am much surprised to observe how deeply his mind was imbued with this doctrine of milk metastasis, in his treatise on diseases of women, or *Handbuch der Krankheiten der Weiber*, &c.

My copy is the 3d edition, 1832. His preface to the 1st and 2d editions bears date, March 15, 1821. I know not the date of the first impression. This writer, it is true, does not adopt the milk dogma in its plenary acceptance, but it is evident that in reasoning on the subject of puerperal fever, and on the suppurations that follow it, he is not delivered from the tyranny of that school. In a note on p. 714, he sets forth his views; and after saying that the *dépôts* do not consist in real milk, he adds, "Betrachtet man die Sache mit ganz vorurtheilsfreien Augen, so gelangt man zu der ueberzeugung, dass Keine Versetzung von wirklichen milch, sondern eine ausscheidung von dem Milchstoffe aus dem blute Statt habe." In this sentence I conceive is contained the admission that among the causes of puerperal fever we are to count certain changes in the blood, the basis of which always is to be discovered in the female power of milk production. In his learned book *Trattato generale di Ostetricia, Teoretica et Prattica*, Professor Asdrubali shows that in 1812, the date of his edition, he was himself one of the last lingering defenders and expositors of the milk dogma; see tom. ii. § 322.

73. Even here, where a sound physiology is inculcated, and educated men ought to be free from the thralldom of sectarian and scholastic tyranny, it still, though buried and forgotten for more than half a century, stalks among us, and sheds a sort of corpse light upon the living body of our Medicine. I myself scarcely find an occasion to converse with my brethren on these topics, without recognizing its presence in the idea that the "blood is poisoned," that it is a disease of the blood, or that the state of the blood, in the case, demands or admits of this or of that treatment. What difference, pray, does it make, whether the said poison is Willis's, or Puzos's, or Doublett's, or Vigarous's aberrant and infarcted milk-particles, or Dr. White's absorptions, which bring about the putridity of childbed fever, or Dr. Robert Ferguson's "vitiating blood," as he calls it? See White of Manchester, 205, and *passim*; and Butter, 342; and Ferguson's whole volume; principles, practice, and philosophy.

74. You shall find that even Rokitansky himself uses such language as this: speaking of puerperal peritonitis, he says, "This disease is often, and even generally, the result of a primary con-

dition of the blood of the female, which predisposes to an exudative process, and is totally distinct from the physiological tendencies of the blood during pregnancy," &c. &c. (vol. ii. 313, Syden. édition). Here is the doctrine, as clear as in any passage of Sellé, or Bichet, or Goubelly; and it now exists in England, where I have been rallied because I was a man who ought to have known that a certain state of the blood, consisting in an excessive product of the fibrinous, or plastic element, or exudative matter, is the real cause of eclampsia puerperalis.

75. There is a funny old author, already mentioned in sec. 35, whose name was Bichet. His book, *Observations sur l'Art des Acc.*, &c., 12mo. Paris, 1758, is an amusing exemplification of curious toadyism; for, being a doctor about the court, he seems ever to stand, as it were, cap in hand, ready to prostrate both himself and his scholarship at the foot of whatever royal or even noble dignity might graciously deign to tread upon his academic neck. He must have had a good business, if we may judge from his accounts of cases.

76. "I was called in," says he, "in 1712, to see a saddler's wife, in the King's *petite Ecurie*. At that time I was practising surgery, though I was also an obstetrician at Versailles. At the house of the sick woman I met Madame de l'Epée, who was then a midwife there, and who told me that, six weeks before, she had delivered the patient, soon after which there came on a very violent fever. The woman was twice bled, and was also purged; but it all did not prevent the patient's leg from falling into the condition in which I found it. I knew at once that it was a *dépôt*, and said to Madame de l'Epée that the cataplasms were of no use, and that the leg must be incised to let out the contained matter. The husband went to beg M. Marechal, first surgeon to the king, to come and see his wife, and I myself called on M. Marechal to bring him to the sick woman, having first made him the dissertation of the disorder. When he had examined the leg, he said it was very hard, and œdematous, with phlyctenæ; and that the leg must be opened in order to try and save the patient's life. When the operation was done in presence of our first surgeon, there escaped about one pint of MILK, in a very corrupted state. His incision extended from the knee to the external malleolus. The

woman recovered in about six weeks, as there was no further cause of milk." p. 226.

77. I wish you would look over Bichet's accounts of his cases, were it but to laugh at the solemn reverence with which he speaks of his little majestic nobility-babies, and of his great patroness, Madame la Duchesse de Ventadour, a lady who was his favorer on all occasions, and most faithfully endeavored to keep her *protégé* clear of all the bad cases in which, haply, his *succés* might be compromised by a failure to cure the disease.

78. I have a great number of those authors who advocated the doctrine of the milk school; and to me it would be a most pleasing task to cite them for you, in their chronological order, with extracts from their pages; but in this work-day country of ours, where every man seems to be spurred forwards by a national rage for business and money-making, I fear I should go too far were I to indulge my taste for citations. Besides, I remember that Mons. Sue, in his *Essais Histor. Littér. et Critiques sur l'Art des Accouchements*, Preface, xxviii., tells us how Mignard, the painter, complaining one day to Ninon de l'Enclos of his daughter's bad memory, "Vous êtes tres heureux," said the lady, "elle ne citera point;" and Sue himself asks: "A quoi donc servent les citations? Le plus souvent pour satisfaire la vanité d'un pedant, et la souffissance d'un auteur." Lest, therefore, I should be accused of such a pedantry or self-sufficiency as Mons. Sue condemns, I shall, for the present, let those old authors sleep in their quiet and almost forgotten graves.

79. In fine, it remains for the Student, after what I have now said, to ponder the question as to what the blood really is; and determine for himself, whether it indeed can become so changed as to take on itself a function as common-carrier for all sorts of things, conveying, like an Adams & Co.'s express wagon, all possible packets and packages to the proper places of a general parcels-delivery, no matter how they get inside of such a convenient omnibus!

80. Our late distinguished and lamented townsman, Dr. S. George Morton, defined "Species, a primordial form;" but no physiologist would dare deny that the specific characteristics of creatures consist in the representation of their combined parts, or

that the parts are as essentially specific, as the whole. If an animal differs from another in its dental formula, or in any other peculiar thing, to that degree as to give it a specific character, it does not derive that specific nature from the place or number of the teeth, or horns, or from its external form alone; but from every item within its whole constitution, which makes its form and substance to be repeated in generation from age to age, endlessly; and there is nothing within it more absolutely specific than its blood—even spermzoons are specific in form; and so are granules of starch, and pollen-grains of flowers; and so are stamens and pistils. Yet, you shall hear scientific people talk of the blood circulating within the living body, and permitting the manifestation of the most perfect health and power, though, all the while, loaded with foreign materials, and modified to that degree as to cause the most extraordinary suddenness, the greatest violence, and an irremissible tendency of disease to destruction of the life!

81. Think, think for yourself, O Student of Medicine! Make yourself a free citizen of the Medical Republic of Letters, that you may know of yourself, and not of another, what is the truth. Make yourself no man's servant or valet, to do his bidding and believe what he believes. The *ipse dixit*, and the *magister sensit*, was for the slaves of Pythagoras, not for American Students. The reproach of our calling is, that we tend to believe everything that is told us, and do not take a stand-point of our own, where we can be fixed immovably. I would rather be a dog, and come to a master's whistle, than be a Scholar with a soul so pliable, that *ipse dixit* or *magister ita sensit* should be the rule of my medical opinions. Truth is everlasting, since it is but the expression in nature and morals of God Almighty's will and power. Truth may be searched into, in many things that respect our calling and mission in the world; and they are but unworthy members of the Scholar-class, who content themselves short of its possession. But I moralize in vain, and shall, therefore, bring to a close this letter, already mayhap too long.

C. D. M.

LETTER IV.

INQUIRY INTO THE REAL STATE OF THE BLOOD IN CHILDBED FEVERS.

82. It is not enough for you to have inquired concerning the opinions of that large section of our class who, in former times, took those strange views as to milk-metastasis, milk-engorgement, milk-infarctus, &c., about which I spoke in my last letter to the Class. You cannot avoid being, to a considerable extent, biased in your practice, by the notions you shall imbibe concerning the state of the blood in pregnant and lying-in women, since much of your etiology, as to childbed fever in particular, will be connected with your views as above stated. I feel impelled, therefore, in discharging the duty I assumed, when I promised to write to you on this subject, to speak still further on the nature of the blood, in this relation; and to that end I now invite you to consider the nature of the Hæmotosis in the fœtus in utero. If our sentiments on that question should coincide, I see not how we shall disagree as to the hæmotosis of the adult; and this I regard as a question of the utmost importance, in whatever scheme of philosophical practice.

83. The physiologists have fancifully compared the absorbents of the body to a sort of uteri, in which the germs of the blood may undergo a *quasi* incubation, before the product is thrown into the general torrent of the circulation at the debouches of the thoracic duct into the vein. It is, without doubt, correct to say, that the elements of our nourishment, whether they consist of absorbed or resorbed molecules, do undergo a process of animalization in their transit along the absorbent vessels—for they there do acquire form, or sphericity, and even a certain reddish or rosaceous tinge, especially in the upper portion of the thoracic duct.

84. I say it is a fanciful thought to compare the absorbents to utriculi, and regard the young chyle-corpuscles as embryo disks of blood, which are soon to be born into the venous system, and then transported with the circulation in various directions throughout the living economy; their development, or education, becoming thereby complete. In this view of it, we might regard each absorbing tube as a little womb, and every corpuscle of chyle as an embryo-globule of blood, to be matured after its birth into the world of the sanguine circulation. Such an hypothesis leads naturally to the admission that the organized blood, or the corpuscular or discoidal blood, consists of both younger and older, or immature and mature globules; but, in either case, we must, for the early incubation of the blood of the adult, look to these supposed vehicles or absorbents.

85. Admitting that this is a just representation, we have still to look elsewhere than in the absorbent system for the power by which the young and new-born disks or globules may be trained, and brought to their most perfect state of development. If the absorbents and their glands do really possess the power of imparting life and organization to the material but not yet living elements found in the chyme, it is not asking too much to invite you to consider that the bloodvessels, also, are possessed of an hæmotosic power, and a power, too, still greater than that of the absorbents, which are only their ancillary portions, or parts provided with a force sufficient to engender only, but not to complete the evolution of the solid constituents of the blood.

86. But I have now to ask you, how is it with the foetus in utero? How is the blood of the foetus prepared and amplified?

87. The embryo, or foetus, as you know, has no point of contact with its mother, save by the placental tufts, which, in a word, are but the capillary terminations of the umbilical arteries, themselves branches of the aorta. In fact, the young embryo commences its existence by extending an aortic trunk downwards, until it divides into two arteries, which are, at that period, its umbilical arteries.

88. Such an embryo has no occasion for primitive iliacs or femoral arteries, for it has no members as yet. The first two branches of the aorta, therefore, are, in fact, the two umbilicals,

which at a later date, are denominated primitive iliacs, and so on, as the arteries come into existence. Now, the distal branches of the two primitive umbilical arteries collected together in the numerous lobuli of the placenta, apply themselves so closely to the living walls of the mother's womb, that exosmose from her vessels, and endosmose into the foetal capillaries, can very readily take place, and a transfer be effected of the mother's plasma, into the venous system of the embryo she is carrying in the womb. And these placental or umbilical tufts are the only points of contact betwixt the mother and foetus. Everywhere else she is defended from such contact by the membranes of the ovum; so that, when the child puts forth its hand, or thrusts out its foot, or rolls in the womb, it cannot touch her, but only its own envelops.

89. I say truly, therefore, that the only thing of the child that touches the mother, is the transitory and deciduous apparatus, called a placenta.

90. All the oxygen the child can acquire, can reach it only by the placenta, and all its nutriment must come into it by the same opening, for the placenta is its only aerating and its only absorptive surface as to the world without.

91. As nothing solid, nothing that has form, can be the subject of endosmose, it is clear that the child can get from its parent nothing but plasma, which is water, containing in solution the proteine and salts, &c., &c. required for its development. It cannot take any blood, but must make every corpuscle for itself, out of the plasma it has absorbed.

92. But, as you have seen, all that enters into it gains access only by the radicles of the umbilical vein, and the whole supply is first cast into its venous system; its absorbing apparatus cannot be said to have anything to do with the function of hæmatisis, as far, at least, as relates to its absorptive force. The resorptive system may, I deny not, have some part to play in the evolution of certain principles of the blood, after the life of the embryo has begun to acquire somewhat of the force of the foetal life—not earlier.

93. But, if the whole product on which it is dependent, is first poured into its veins, and thence into its heart, arteries, capillaries and so on, in the circulation-round, you cannot escape, nor even

evade the conviction, that the power of Hæmotosis is a power attached to the bloodvessel tissue ; and, if it be so as to the foetus or embryo, it must be so as to the child, the youth, and the man. I am not going to discuss the question whether blood is made in the lungs, as some aver, or in the liver, as others say, or in the spleen, &c., &c. It is a sufficient answer to all such propositions, that the blood resides within the bloodvessels, and that, whether they be pulmonary splenic or hepatic bloodvessels. To deny that the Hæmotosis is a bloodvessel force, and say that it is a pulmonic force, &c., is but to elude, and not to meet and answer the question. Neither ought any man to say that the blood is developed or evolved from the primary but dead elements of it, by oxygen ; for all the oxygen in the universe of God could not compound one small corpuscle of blood, except its material elements were first brought together within reach of the inductive power of a nervous mass, in an endangium, by whose force to be evolved.

94. I do not say a nervous mass, consisting of a cerebro-spinal axis and its dependencies, but a nervous mass, even if considered in the state of what Mr. Oken called point-substance. Are not even the living elements of the egg capable, under incubative heat, of inducing into the elements of the blood of the chick in ovo, neurosity of a force sufficient to cause them to assume the form and qualities of blood, such as we observe in the *punctum suliens*? It is therefore in the highest degree unphilosophical to reason as to the power of oxygen, or to the liver or spleen, or to glands alone, as the cause of Hæmotosis, since oxygen is only one of the conditions necessary to the act, while nervous induction from the nervous mass, is a condition equally important and indispensable.

95. But what do you mean, Gentlemen, when you pronounce the word bloodvessel? Do you mean to express the idea of the elastic wall of arteries, or the muscular fibres with which certain parts of vessels are provided? In my opinion, the concise idea of a bloodvessel is that which we have when we speak or think of the *membrana vasorum communis* (Endangium); for that is the tissue to which appertains the vital property of the vessel, as relative to the mutual action and reaction between the living solids and the fluid blood. Muscular and areolar and elastic walls

of bloodvessels do not contain or transmit that nerve-power by induction, which is essential to the formation and preservation of the blood in a living state; they are not the indispensable parts of the bloodvessel, for every sophomore knows that when a bloodvessel penetrates within an organ, to do its physiological-work there, only the Endangial membrane goes in, leaving all its accompaniments outside.

96. Burdach, *Phys.* vol. vii., p. 88, says: "The blood is the totality of the substance of an organism under a liquid form;" and Prof. Oken, *Phys. Philosophy*, p. 354, says, sect. 1994: "The blood is the fluid body;" and in sect. 1995, "The body is the fixed or rigid blood." The antagonism or polarity of these two is a vital force—either dies but for the continued activity of the other, and this relation of each to the other takes place through, and only through the solids of the bloodvessel (Endangium).

97. Allow me to cite for you another passage from Burdach, vol. vi., p. 194: "A vessel is the special delimitation of the vital juice constituting a liquid apart and distinct from all other humors; that is to say, the blood: it traces out the path it is to pursue, and marks the direction it takes. It may be regarded as the expression of the blood in space, for it was formed by its current, and is one with it. Hence, it follows that the most essential part of the vessel must be in immediate contact with the blood, and constitute the main layer of its walls. This internal membrane (*membrana vasorum communis*, *Endangium*) extends uninterruptedly throughout the heart, the arteries, the capillary vessels, and the veins," &c., &c.

98. This is the true bloodvessel—it is the solid through and by which all the induction of life, or nerve-force, into the elements of the young blood is effected, in order to raise them to strength and perfection, and it alone sustains the life of the older or maturer globules. Hence, a condition of that induction is, that the blood should move from space to space within the inducing or Hæmatomic surface. To rest is to die; to cease to be endowed with oxygen is to die; and to be removed from the presence and influence of the Endangium, is equally sure to bring about the death of the blood, which lives in and by it, as its perpetual,

watchful, particular providence and protection, its life-giver and sustainer.

99. The blood-globules being of various ages, and various degrees of perfection, their material elements are first passed, by the power of nerve-induction, from death to life, and, when that power is withheld, from life to death. The vital state of the blood is, therefore, in constant dependency on the vital state of the Endangium. Is that tissue healthy, the blood is so likewise; is it sickly, exhausted, dying—the blood changes its phases along with those of the power it depends upon. Is the Endangium struck with palsy in a limb, or in a larger part of the body, as in paraplegia—it is dangerous and noxious to the blood, to be driven within the vessels of such a deranged and diseased structure.

100. Many of our most fatal childbed fevers are nothing more nor less than instances of endangitis; cases, in which the membrana vasorum of the uterine and crural, and even the cava veins, are the seats of an inflammation called childbed fever, milk-leg, depot laiteux, &c. &c.

101. A woman pregnant, is often observed to labor as to her circulation; she becomes sometimes plethoric and hyperinotic; at other times she is to the last degree hydræmical. The force of her hæmotosis is exaggerated or exhausted, as the case may be, and the direct fault, the pathological fault, is to be traced to a state of the Endangium, which is her blood-making tissue, her blood-membrane, and which has the same relation to the function of hæmotosis, as the gastro-enteric mucous membrane has to the process of her digestions. If the digestive organs become diseased, the power of digestion falls proportionally; if the hæmotosic tissue becomes diseased, the hæmotosis fails in like manner.

102. We habitually speak of the digestive mucous membrane, and of the respiratory mucous membrane—blood-membrane is equally a true and honest word, one that conveys an accurate and concise meaning or idea. Well, then, when I speak to you of blood in diseases, as depraved, as vitiated or dissolved; as ruined and incompetent to carry on the functions appurtenant to it; as the antagonism of the solids; I desire you to understand me as speaking of diseases of the membrana communis (Endangium). I

do not more conclusively refer the dysenteric diarrhœas, eroups, &c. to a state of the gastro-intestinal, or pulmonary or laryngeal mucous membrane, than affections of the blood to affections of the blood-membrane, Endangium. All scarlatinas, measles, variolas, varicellas, erysipelas, gout, rheumatism, and many forms of childbed and other fevers, have their prime seat in the blood-membrane (Endangium), and are but so many varied expressions of its diseased conditions.

103. This proposition I offer to you, knowing that it will excite a smile of contempt for me in many a self-sufficient physiologist. Still, it is one so true, so useful, so enlightening to the path of the practitioner and the researches of the pathologist, that I am ready to meet with the scorn, which from many a professional martinet it cannot avoid and draw down upon me, as a medical dreamer full of wild theories and unsustained assumptions. Time will show whether our class have not, for more than twenty centuries of studies and observations, neglected to look into the pathology of the membrana vasorum communis, a tissue that has perhaps more extensive and important relations to etiology in our cardinal fevers, than even the digestive or the pulmonary mucous membrane, or both of them together.

LETTER V.

IS THERE A POISONED STATE OF THE BLOOD?—OF CONJECTURAL PUTRIDITY—VITIATED BLOOD THE CAUSE OF CHILD BED FEVERS—IN WHAT WAY THE BLOOD CAN BECOME CHANGED—IT IS A DEPENDENT OF THE BLOOD-MEMBRANE IN DISEASE, AND NOT VICE VERSA.

104. WHAT difference does it make to you, whether our Class shall receive and adopt the milk-dogma of Thomas Willis and the Frenchmen, from Goubelly to Puzos, and from Doublet to Vigarous; or whether they shall prefer to fill the blood with various products of absorptions and resorptions, and then attribute to the noxious presence of those putative foreign matters, a host of diseases, whose evolution is made by them to depend upon them either alone or in chief? I repeat that you are doomed on many future occasions to fight with the shadow of the milk-dogma of the eighteenth century. If you will take up Alexander Gordon's Treatise on Puerperal Fever, you may even there see that, although he says nothing about milk-metastasis, his mind was clouded from first to last with dreams of *putridity*, of which he saw the pretended signs in the appalling weakness of his patients. It was not until he had dissected the body of Mrs. —, No. 38 of his dark early list, that he became aware it was an inflammation he had been treating—I say, "aware;" and, perhaps, I ought scarcely to say so, since, in a subsequent page, he observes that "the disease, which was inflammatory in its beginning, becomes putrid with progress. The source of the poison is in the cavity of the abdomen, for which there is no antidote in the *Materia Medica*. . . . This deep-seated poison cannot be corrected in any other way, than by being carried out of the body," etc. etc.

105. Poison! Antidote!

106. What poison? Do you hear Gordon speak of a deep-seated poison in the belly which has no antidote in the *Materia Medica*? Do you know this poison? I am sure, for one, that I am wholly ignorant of its nature, and that no person has ever analyzed it, or even seen it, though Dr. Thos. Douglass (*Dub. Hosp. Reports*) says, he could smell the effluvia of puerperal fever! It has never appeared, and we say *de non apparentibus et de non existentibus eadem est ratio*. The poison was in the mind of the observer. But Gordon is not alone in his views.

107. Dr. Charles White, of Manchester, p. 213, says: "It does not appear that this disorder can be ascribed to simple inflammation. The patients complain chiefly of tension, soreness, and tenderness of the belly, and are seldom affected with that excruciating pain which usually attends common inflammation of the bowels; but it evidently manifests itself to be of the *putrid* kind, occasioned by human effluvia; by the accumulation of acrid *putrid* bile, and a putrid colluvies through the whole intestinal canal and organs of generation, and is a malignant fever of the same genus as the jail or hospital fever."

108. Dr. Rigby, of London, in an article beginning p. 482 of vol. vii. of *Brit. and Foreign Med. Review*, notices Dr. Robert Ferguson's Essay on the Most Important Diseases of Women. Hear Dr. Rigby: "In a notice of Mr. Moore's work in our second volume, we ventured to express our own belief that the affections which we ourselves considered as especially entitled to the name of puerperal fever, belonged to that class of diseases, the fundamental character of which is a morbid condition of the blood, produced by the introduction of some deleterious agent into the circulation." There you have it! This is in Dr. Rigby's own words. The blood is poisoned by "some deleterious agent!" Suppose it had been Puzos's or Doublet's milk, or Thomas Willis's ferment! what is the difference to the poor woman? In his *Midwifery*, Philadelphia edit., p. 417, the same gentleman observes that, "although we cannot quite coincide with the views of Dr. Ferguson to their fullest extent, respecting the exclusive cause of the various forms of puerperal fever, viz: the vitiation of the fluids, still, in a great mea-

sure, we consider them as correct, having not only taught them for many years, but published them in our lectures on this subject in 1835." . . . "We cannot agree with the opinion that the vitiated state of the blood is the secondary and not the primary link in the chain of phenomena (p. 423)." What a strange opinion! A man holding such a tenet as that, must surely be one who regards the blood as a self-producing substantiality, and wholly independent afterwards of the solids. Yet the same person, if interrogated concerning his philosophical opinions as to the hæmatisis, could readily be cross-questioned into an admission that the blood-making power is one of the principal attributes of the special vital solid charged with that high and indispensable function.

109. Dr. Ferguson, in his work, p. 53, has this formula:—

- "1. The phenomena of puerperal fever originate in a vitiation of the fluids.
- "2. The causes which are capable of vitiating the fluids are particularly rife after childbirth.
- "3. The various forms of puerperal fever depend on this one cause, and may readily be deduced from it."

110. Denman, in his doubting recommendation of venesection in the treatment, urges the necessity of great promptness in the use of the method, as signs of *putridity* early appear; and Dr. Hulme, p. 112, says: "As the puerperal fever hath a strong tendency to run quickly into a state of *putrefaction*, all causes during pregnancy," &c.

111. It is an easy thing to multiply the citations concerning *putridity*, as to childbed fever, but I shall not overload these pages with them. It is enough, one would think, to quote those above, to set you to pondering upon the possibility of *putridity* in any living body. A man may have a foul and even putrid sore on his surface, or, in confluent smallpox, the ooze from innumerable pustules may make the man resemble a living mass of corruption; but putrid blood in the vessels, and heart, and brain! Surely, the word putrid does not, nor cannot express the meaning of the speaker! Such a word is a traitor to the speaker of it; in plain English, it is a false word. Does

it not mean the same thing, under a different name, as milk in the blood, *errement de lait*, &c.?

112. The notion of putridity in the blood, is surely not very different from that of milk fermented in the same fluid. But of these ferments we know nothing. It is only words we use, and the meaning of them is vague and imprecise. Some persons suppose that substances may find admission into the recesses of the body, that are neither spores nor ova; but, like spores and ova, capable of evolution within, to the destruction of the vital powers, and even of the organization. Others insist that spores of vegetables, which are cells, obtaining admission, produce cells within cells, to the utter subversion of the texture and the life. We know nothing of them; we do not see them, and cannot prove their existence—nor, indeed, if we could do so, would they be thereby proved to be the causes of Zymotic disorders.

113. After a careful examination of the opinions and suggestions of many of the most distinguished and acknowledged good authors on the subject, I cannot bring myself to believe that because a woman is pregnant, or lying-in, her blood may lose its specific character and become something else—which is not blood, but which still carries on the functions of the blood, and either destroys her life through childbed fever, or, after bringing her down to the gates of death, instantly, in the twinkling of an eye, ceases to trouble her, and allows the recovery of the most perfect health, even in a very short time. It is not to be doubted, that multitudes of women laboring under childbed fevers, even of the worst character, have been cured by a simple venesection, a *jugulare febrim* bleeding, that, at a single blow, has struck down the power of the inflammation to advance one line beyond its actual area, and at the same moment impressed upon it a sure tendency to disappear rapidly by resolution. I have seen and treated many such a case. Where was the poison, where the putridity, that were thus cured or removed by our good bleeding at the arm!

114. I am anxious that you, as Students of my Class, should, if possible, be educated in absolute freedom from all shackles of dogmatism. It is a dogma to say that the blood is poisoned, and that an entity foreign to the body is within it, exercising on the

organism its pathogenic force. I say it is a dogma, and a dogma is only an "I think." To think is not to know, but only to believe: a dogma is a settled opinion; but a settled opinion is not a knowledge, but only a belief. Now, as to foreign matters in the blood, you can only think them to be there, you cannot know it.

115. If there is a foreign entity in the blood capable of self-multiplication, as by fermentation or the evolution of cells, why then, it might follow, that the diseases so developed by them should be contagious; but if the childbed fever is not a fever, but only an inflammation, then it cannot be transported or communicated by contact or combination, or *ad distans*.

116. Many men say, and even insist, that childbed fever is contagious; and I consider it quite probable, that if you could pass the question to a vote in this country, much the larger portion of the forty-two thousand American physicians would vote in the affirmative; and that they would be sustained by a similar sentiment of the public at large. I have been dismissed or released from my engagements to superintend the confinement of certain of my patients, because either they or their friends became apprehensive of the contagion I might carry from puerperal-fever patients that I had not under care; for the rumor of epidemic childbed fever was so terrifying, as to absolve those families from the obligation of treating their physician with respect, or even politeness. My conversations and my intercourse with the brethren, convince me, too, that even where the individuals have been highly educated, there is a natural disposition among the medical men to adopt the convenient hypothesis of contagion; for the far greater majority of my personal acquaintances think that an accoucheur, who has the misfortune to take care of a case of puerperal peritonitis, is scarce fit to be trusted near a pregnant, parturient, or lying-in woman.

117. We ought to reason together upon this subject, as one that deeply concerns our feelings and our interests as well as our duty; for if there is a shadow of reason why we should adopt the belief in contagion, we should make haste to discover the truth, and be guided by it in our conduct. I do not wish to say anything in this letter to excite anger, or lead my reader to think me

deficient in respect for other men's opinions. Yet I cannot refrain from expressing the surprise with which I behold the indifference with which the most important opinions are taken up or laid down, and particularly, on this subject of contagion, which as to childbed fever appears to me to have no shadow of reason in its behalf.

118. But while the opinion of childbed fever contagion ought, in my view of it, to find no supporters among truly educated medical men, I am well aware that you are to be left to the operations of your own judgment in adopting or rejecting it; nor have I the least desire to persuade you to think, because I think; I only wish you to think because you have inquired into and discovered the truth of the matter.

119. To me personally, it is nothing, that you should believe this or that. The direction of your opinions interests you, and that portion of the people who are to confide, in their distresses, in your sagacity and knowledge, and in an important degree also, the profession of which you are members; for what you do or say in this matter cannot but have influence, greater or less, on some parties, who will be misled by your erroneous opinions, or correctly informed by your truly enlightened and just views of the subject. I hold it to be the duty of every man of us to endeavor, at least, to do something towards spreading abroad the truth; and so adding, even if it be but one poor mite to the Treasury of Science. Your opinions, therefore, do not interest me personally.

120. If what I think is truth on this subject, it is no truth of mine; it is nature's own truth; and I have, therefore, not the least inclination to find that any Student of mine shall believe, because I believe, this or that. Far from it—I heartily despise, and deny his claim to the Scholarship, any man who makes himself the slave of other men's opinions; for a Scholar can only be truly a Scholar by becoming a freeman in the Republic of Letters, holding all his rights in knowledge and science in absolute fee simple, so that what he has is his, and not another's.

C. D. M.

LETTER VI.

CONTAGION IN CHILDBED FEVER—DEFINITIONS—CONTAGIONS ARE BOTH SPECIFIC AND INCUBATIVE—CONDITIONS OF THE PROBLEM—COMMON TENDENCY TO BELIEVE IN CONTAGION—DR. HOLMES'S PAMPHLET—TONELLÉ'S, BAUDELOCQUE'S, JACQUEMIER'S, AND KIWISCH'S VIEWS—VON BUSCH AND HIS HOSPITAL-EPIDEMIC—COLLINS'S, DUBOIS'S, AND SCANZONI'S OPINIONS—DR. CAMPBELL'S EXPERIENCE—AUTHOR'S EXPERIENCE, WITH A TABLE—RUTTER'S CASES—GOOCH A CONTAGIONIST, AND ARMSTRONG, ROBERTON, AND CHURCHILL.

121. I JUST now opened *Stephens's Thesaurus Linguae Latinæ*, to learn what is his interpretation of the word Contagion, which he derives from *Contingo*, *tigi*, *tactum*, *egêre*, which he says is, *idem quod continuatio*; and after giving as usual an infinite number of derivations from the verb, he remarks of *Contagium*, that it is *pro contactû*, and means *genus morbi qui ex contactu contrahitur*, a species of disease contracted or taken by means of touching. Stephens cites many instances of its use by authors.

122. Dr. Thomas Willis, *de Febris*, p. 224, says that by the word contagion we mean that force, or those acts, by means of which an affection existing in one individual, may excite a like affection in another individual; and that this effect may be produced either immediately by contact, or mediately, and *ad distans*; as if, the disease being in a house, the poison should be transported thence into another house; or, as if, a person, by touching any clothing, &c., that had been left in an infected place for many weeks, months, or even years before, should contract the plague by such touch.

123. In explanation of this faculty of contagion, Willis suggests that from all bodies whatsoever, there continually escape

effluvia which envelop or surround them with a nebula or haze, and clothe them as down of a peach invests the fruit; an idea, which, according to him, is received by philosophers as one of the truest propositions. In this manner, he thinks, pestilential poisons may be supposed to arise, and escape as effluvia, or vapors; and being specific in character, are capable of reproducing the same specific diseases, whose existence, in a body, caused that body to create and eliminate them.

124. Consider now these views of the celebrated English physician, and inquire into their reasonableness; and, further, determine, for your own satisfaction, whether a woman, who labors under a childbed fever which is either a metritis or a phlebitis, can develop a metritis or phlebitis in another pregnant or lying-in woman, and in no other human being besides; and more than that, whether a physician, or a nurse, can carry Willis's nebula or halo in his hair, his dress, or his hands? Such are the inquiries you are to make and to answer: You must answer them.

125. As the word contagion is in constant use in our calling, one ought to know what he means to say when he employs it, and should, therefore, know its value as the sign of his idea. What is the value of material idea excited in you, when you used the word contagion?

126. Krauss, whose learned lexicon is, I presume, much to be depended on, says of the word *contagion*, that it is *poisoning or infecting by a touch*; and Dr. Dunglison, in his *Medical Dictionary*, uses these words: "Contagion is the transmission of a disease from one person to another by direct or individual contact. The term has also been applied, by some, to the miasmata arising from dead animal or vegetable matter, bogs, fens, &c.; but in this sense it is now abandoned."

127. Webster's *Dictionary* tells us that, "contagion is literally a touch, or touching. Hence, the communication of a disease by contact; or the matter communicated; more generally, that subtil matter which proceeds from a diseased person or body, and communicates the disease to another person, as in cases of smallpox, measles, anginas and malignant fevers, diseases which are communicated without contact. Also, pestilence, a pestilential disease, venomous exhalations."

128. Nacquart, in the *Dict. des Sci. Méd.*, says, under the word contagion * * * it is proper to call by the name of contagion, the mode by which diseases are transmitted from one person to another, by mediate or immediate contact. Contagious, *contagiosus*, susceptible of transmission by touch.

129. I think the above citations may suffice to show that whenever you use the word contagion as of contagious diseases, you imply disorders, produced, whether by means of actual contact of persons or by means of effluvia, nebula, or halos, which, evolved by an individual, and enveloping him like an invisible mist, may be carried by him from place to place; or surrounding, or adhering to clothing, &c., may poison other people, and so reproduce, not all sorts of diseases, but the very same sort of disease which gave rise to it in its specific nature and attributes. If you will use the word contagion, I think you are bound to restrict it to its real sense and meaning.

130. If, as some pretend, childbed fever is a fever indeed, and not a mere topical inflammation that gives rise to febrile phenomena by means of the irritating power of the local disease, there might remain some chance of a probability that such an affection should be a contagious one. I know not what ideas you may have imbibed upon these points, but it is the object of this work to prove that childbed fever is a simple state of inflammation in certain tissues of pregnant women, and of women lately confined, and that the fever that attends it is a natural effect of intense constitutional irritation from the local disorders. So far as I have been able to investigate the subject by reading and by clinical observation, as well as by necroscopic researches, I rest deeply convinced that the fever does not take the initiative, except in very rare instances; but on the contrary, that an area of inflammation being first established, the reactions ensue thereupon; and I beg you here to observe that, in all the truly contagious disorders, the constitutional affection leads the train, and brings on the topical lesions after an indispensable preliminary incubation.

131. Do not suppose that I am ignorant of the opinions of eminent persons, who do not view the matter in this light, but conceive of the disease as a real fever, and that some most celebrated writers, even among those who were never slaves of

the milk dogma, have insisted that there is no inflammation in the case, in most epidemics. Maximilian Stoll asserts these opinions in his articles on puerperal fever in the *Ratio Medendi*, 211. I do not wonder at this circumstance, when I reflect that childbed fever is often an epidemic disease that spreads over whole districts and even great countries, for it is a very difficult thing to conceive of a purely topical affection as governed by the force and laws of epidemics.

132. Contagious diseases, it is worthy to be observed, are always specific in character. Thus smallpox-poison or contagion, develops only smallpox, and never measles, or scarlet fever, or jail or yellow fever, or intermittents, or spasmodic cholera. Whenever the virus acts at all, it can only act in one line, in one specific way. The same is true of syphilis and all other truly contagious diseases. As to contagious diseases, further, they affect the race of mankind without respect to age or sex of those who become exposed. There is no presumable exemption among men. Of smallpox, Willis says; we are all predisposed to it: *quæ nimirum prædispositio naturalis, homines quidem solos omnes, idque semel huic morbo obnoxios reddit.*" 254.

133. In a case so specific as this of smallpox, there can be little hope to deny, on philosophical grounds, the absolute contagiousness, which is also clearly discoverable by our empirical knowledge of the facts, as in contagion by contact, *ad distans*, and by inoculation.

134. If one of the conditions of contagion is that it is no respecter of persons, but attacks all individuals alike, you should consider whether one of the indispensable conditions of the proposition, that childbed fever is contagious, is not wholly wanting; childbed fever, attacking not women only, but only puerperal and parturient women. How comes it then to pass, that a mortal virus or contagion should have power over a woman who is pregnant, or recently delivered, while it is innoxious for all others in the world? Do you say that there is, in nature, a contagion of intense virulency as to the human body, only, when in the exercise of the highest, the most culminating forces bestowed upon it by the Creator? You know already that in some hospitals where every parturient dies with the epidemic, no harm falls on the

nurses or the physicians, and this too in a house filled with a contagion, terrible as plague, and more mortal than variola! Come now, think of this! ask, how can these things be? why should it attack the pregnant or in-lying woman alone? Is such a creature not a woman still; still a member of the race; still under the law of species? Hath not a woman eyes? hath not a woman "hands, organs, dimensions, appetite, affections, passions; fed with the same food, hurt with the same weapons, subject to the same diseases, healed by the same means, warmed and cooled by the same winter and summer" as a man is? "If you prick her will she not bleed? If you tickle her, doth she not laugh? If you poison her doth she not die?" Why, then, do you say that, being, in all her physical attributes, and in all of her sensibilities, one of the race, yet she is in this unlike to her kind; that a contagion, mortal as nicotine, and speedy as prussic acid in its processes, is absolutely innocuous for all her race, except for her alone, and for her also, except she be pregnant or recently delivered? Not only must she be pregnant, to subject her to such influence, she must be well advanced towards her term; for my own experience herein coincides with that of Kiwisch, to show that early pregnancy and abortion rarely afford subjects for childbed fever; though it is true that some of the cases related by Hippocrates, as well as by more modern writers, are of a very early date of the gestation.

135. If you could make out the case that pregnant women are, without exception sick women, which you cannot do, then you might, with some semblance of reasonableness, come and declare to all the world that pregnancy, being a pathological state, it contains in its very fact the rationale of this susceptibility to a poison fatal as arsenic. But, I repeat, you cannot say so; and further, in an epidemical season, it matters not who is the woman or what the state of her health that goes to the pest-house called a lying-in hospital, to be confined, she meets the cause there, and there it seizes upon and destroys her.

136. It is an absolute condition of contagion, that it must undergo some certain incubation. If it is a ferment, it must have time to ferment; if a spore, it must have time to develop spores; if infusorials or infusorial ova, there must be time for incubation.

Such is the law of variola, vaccinia, measles, varicella, &c. &c. Is this so? Yes? Then read this case from Tonnellé, *Archives Générales*, 22, 360. "Obs. II. *Puerperal fever, with Uterine Phlebitis; rapid progress.* Marie Cons —, 28 æt.; of an excellent constitution; happily delivered at the Maternité, Dec. 26th, in the evening; was well the next morning, the 27th; but had some vague pain in the belly in the evening of the day. On the 28th, in the morning, the pains became more violent. The lochia, at first abundant, were now suppressed; the face pale, with changed expression; tongue dry; pulse small, contracted, and frequent. She had a hip bath, and fifty leeches on the hypogaster. The leeching, though plentiful, did not lessen the pains. During the day she was very anxious and agitated. In the evening she was delirious, and fell into a deep stupor. The skin soon became covered with a viscid sweat; the extremities grew cold, and she died in twenty hours after the commencement of the disorder." The dissection disclosed the most extensive suppuration of the parts within the pelvis.

137. Now tell me whether this was a contagion, and whether, if you had given her a dose of arsenic, you would have killed her quicker! Contagion, indeed! A man must be very badly informed to say so. Kiwisch, *Klinische vortrage* 1, *Abth.* 529, says: "The course of a case of childbed fever is so rapid, that there are few cases of epidemic disorders that hasten with such speed to their termination; and we have observed one instance, in which it completed its course by destroying the life of a woman, confined after a natural labor, in the short space of eight hours." Is Kiwisch a man of truth? Then what have you to say to such a contagion as this?

138. Many a woman has entered upon her labor in apparent health, and has scarcely given birth to her offspring before she was attacked by metro-phlebitis, and then been deprived of existence within less than twenty-four hours. Forty-eight hours is frequently time enough to effect the dissolution. Are these inoculations? Are these contagions? Are these infections *ad distans*? It is nonsense to say so.

139. I hope you will not answer me saying that facts are stubborn things, and facts show it to be contagious, for women do fall sick with childbed fever, provided they be attended by a

certain physician, or a certain monthly nurse; whereas other women, waited on by any other physician or nurse in the same town, street, or district, shall not be attacked at all; nor would these poor women suffer, had they the good fortune to fall into the hands of people who should not carry contagion with them wherever they might go!

140. Now here is a *caret* in the series of your conclusions. You are not believing, but you are going further: you know that childbed fever is contagious, because Dr. A meets in his practice with seventy cases of the disease, while Dr. B, an equally busy man, does not encounter a single one, though they cross each other's path every day. Dr. A's track is marked out by victims, while there are no traces of Dr. B's path, except it be in recovered women. And you reiterate that facts are stubborn things. Yet you do not know, you only infer and suspect, or surmise that, if Dr. B had taken charge of A's cases, and *vice versâ*, the result would have been the same on the whole, only the dead women would have lived and the others would have perished. Who told you so? You believe so. Well, I have some small respect for your belief, while I should bow reverently to your knowledge—you believe so—I don't believe so at all.

141. You have reasoned badly; and it is true, as Cuvier has spoken the words, that "it is only when the laws of General Physics and those that result from conditions of existence are exhausted, that we are reduced to the simple laws of observation." Cuvier: *Regne animal*, Introduction, p. 6. In saying "facts are stubborn things," you rely on simple observation, and through it have reached the conclusion that Dr. A's patients were poisoned by him, and Dr. B's not poisoned at all; in doing so you have laid aside a method of reaching the truth, far more sure and certain than your empirical one, I mean that of comparing the conditions of existence of the problem.

142. What are those conditions?

1. The patient, a woman, is a human being.
2. She is assailed by the most malignant of contagions, and which can destroy within twenty, or even within eight hours—as precipitately as cholera, yellow fever, or Syrian plague.
3. She is pregnant, or recently delivered.

4. No human being save a pregnant or parturient woman is susceptible to the poison.
5. The poison is developed by the body of the living but sick woman.
6. It is developed with almost instant activity.

143. Now, out of these six conditions of the existence of your proposition, every one of which is indispensable, you must give up No. 5 and No. 6. You cannot maintain an hypothesis of this kind, especially as relative to the 5th and 6th propositions, for you are compelled, in your contagious maladies, to allow of an incubative stage, but here is none. Old John Fernel knew better than this, for in his *Universa Medicina*, 8vo., printed at Utrecht in 1656, he says, at p. 508 of the *Libri Quatuor Posteriores*: "Id intro subiens, spiritus, sanguinem, humores, partisque substantiam contagione labefactat; hinc sensim serpit longius in partes principes, sed tam segniter, ut raro ante diem vigessimum, interdum non nisi anno symptomatis se prodat." He is speaking of the contagion of rabies. If you will be contagionists, I wish you would join in Fernel's wish, where he says: "*Velim tamen insuper mihi aculeos ex animo evellas, quos non nulli infixerunt.*" You must give up No. 5. and No. 6 as to all first cases, because the woman could not take it from the physician or nurse, when neither the one nor the other had approached a case.

144. Further, childbed fever, say you, is a specific contagion. It cannot therefore have multifarious sources; it is developed by a living being, and transferred as a nebula, or a substance, from place to place. Do you mean to say that a healthy woman, who, falling into labor, gives birth to her child in a natural way, afterwards creates and dispenses a miasm or ferment, a germ, a spore, a cytoblast, an ovulum, that, reproducing themselves, can poison and destroy other women? If you say so, then what other sources are there that produce these entities in any first case of the disease? You are inconsistent with yourself, for you say that the specific poison of childbed fever is a something evolved by the vital force of a sick woman; not a woman in health: where, then, I repeat, shall we go to seek for the source of that specific thing that made this well woman sick first in the series of cases? She could not evolve the poison or miasm

when she was in health; but according to you, this disorder of hers is specific; that is to say, it is produced by a certain, always identical cause.

145. You argue that the disorder is contagious because many women are observed to fall victims in the same hospital, or the same ward, or under the care of the same practitioner or nurse; and you say with an air of authority, that "facts are stubborn things." Very well, let it be admitted that facts are stubborn things, but let us interpret them aright and not falsely. Let us not fall under a delusion to believe, as St. Paul says, a lie. But, above all, let us not, in advocating a particular interpretation, become factious, and fall foul of all others except those who swallow our orthodoxy as to science and conduct. Let us not "assume the God, affect to nod," and shake the spheres with our voice potential.

146. An American writer, in a small pamphlet essay on the contagion of puerperal fever, assures the Republic of Medical letters that he would not be "understood to imply that there exists a doubt in the mind of any well-informed member of the medical profession, as to the fact that puerperal fever is sometimes communicated from one person to another, both directly and indirectly." Such is the text of Dr. Holmes's paper on the subject; yet our celebrated townsman, the late Prof. Dewees, whose writings in obstetricy gave him a world-wide fame as a well-informed member of the medical profession, at p. 420, *Treat. on Dis. of Females*, uses the following language: "In this country, under no circumstance that puerperal fever has appeared hitherto, does it afford the slightest ground for the belief that it is contagious."

147. Dr. Holmes does not agree as to *doubts* with Dr. Robert Lee. "The facts I have observed," says that distinguished writer, "though they have led me to adopt the opinion that the disease is sometimes communicable by contagion, and sometimes has a connection with erysipelas, have not, perhaps, been sufficiently numerous, and of so decisive a character, *as to dispel every doubt* on the subject of its contagious or non-contagious nature, and prove that it is a specific inflammation."—*Lect. on Mid.* p. 489. You see that Dr. Lee almost doubts and almost does not doubt.

148. That M. Tonnellé was a "well-informed member of the medical profession" is proved by his celebrated paper on puerperal fevers in the *Archives Générales*, Nos. 22 and 23, and by universal consent. Now he asks, at p. 349, of tom. 22: "Shall we have recourse to contagion to explain the development of these affections? Not more than to the foregoing; for, with but a little knowledge of the arrangements of the hospital wards, it is out of the question to admit this (contagion) cause. A single infirmary collects together all sorts of cases of disease following lying-in; along with the gravest puerperal fevers are found the lightest disorders. Such a state of things is certainly fit to favor contagion, yet we have never seen anything that should lead us to believe in it. Moreover, women recently delivered, notwithstanding they were placed in separate wards, took the disease; indeed, here it is the ordinary rule for them to take it, being often attacked immediately after labor and before they have had any communication with the other women."

149. Dr. A. C. Baudelocque, a well-informed member of the medical profession, and whose admirable work on puerperal fever was crowned by the Medical Society of Bordeaux, says that although, in the actual state of our knowledge, it is not possible to dissipate the prevailing uncertainty on the subject, "I nevertheless, in spite of the assertions of several respectable English physicians, for example, Joseph and John Clark, am in favor of the non-contagionists."—*Traité de la Peritonité Puerp.* 127.

150. Dr. Jacquemier, one of the latest as well as most intelligent authors on midwifery, says: "I do not invoke," as rationale of the invasion, "contagion, because it seems absolutely foreign to the development of puerperal fever, as well when it prevails epidemically as sporadically."—*Manual des Acc.* 2 vols., Paris, 1846, p. 660, vol. 2.

151. Kiwisch von Rotterau, whose early and most lamented death has robbed German obstetrics of the second Wigand, and whose brilliant intelligence allows him a place in the highest ranks of our Scholar class, was a man so extraordinary that I will not forego the pleasure of presenting to you some extracts from the biographical notice of him by his friend, Dr. Halla, which is printed in the *Vierteljahrsschrift für die Heilkunde*, ix. Jahrgang, 1852.

152. Franz Kiwisch, Ritter von Rotterau, was a native of Klattau, in Bohemia, where he was born April 30, 1814. At the age of twenty-three, after five and a half years of earnest Student life, he was admitted, in 1837, to the degree of doctor of medicine, and a few months later to that of doctor in surgery. He then entered the Prague Lying-in Hospital, as clinical adjunct, where he remained two years, gathering an immense amount of experience and learning in all matters pertaining to his professional pursuits. He subsequently travelled in Denmark and Sweden, and then spent some time at Paris and London. After returning from these travels, he gave lectures, at Prague, on diseases of women, which added greatly to his reputation, and contributed mainly to the spreading abroad of knowledge on those subjects throughout all Europe. Those who read his admirable *Klinische Vorträge*, know how well this fame was truly earned and highly deserved. In all this career he was connected with the best of that bright galaxy of learned medical Scholars, whose names are now constantly repeated in the progress and prospects of our literature. In 1845, K. removed to Wurtemberg, where his great reputation attracted crowds of visitors who came to seek his health-giving counsel, and it was here he received pressing invitations to St. Petersburg, from individuals of the Imperial family. The death of Prof. Jungmann, of Prague, opened a higher career for Kiwisch, who returned to the Bohemian capital, to the great joy of the profession there. His excessive labors, and all-sacrificing inattention to his own health, led to a series of maladies, which removed him from the world, of which he was an ornament, on the 29th October, 1851.

Let me now proceed to show you what were the sentiments of this extraordinary man on the question before us. After arguing the subject at length, he adds: "Let us assume a readily communicable contagion as the sole, or even as the chief cause of the sickness, yet I cannot comprehend how it shall happen in most of the lying-in institutions, where a free communication goes on among the inhabitants, that the attacks will suddenly cease to take place, and again break out as suddenly, so that the most decided contagionist ought to be driven to the conviction that such disorders do depend upon some local origin, or atmospherical influence."—*Klin. Vorträge*, ii. 583.

153. Such are the words of the illustrious Franz Kiwisch; and though they alone, ought to suffice to show that some well-educated physicians do entertain doubts on the question of contagion in childbed fever, I cannot help calling your attention to the following striking relation, which, it seems to me, might be enough to end the controversy, if anything can end it in the present age of the world.

154. The *Neue Zeitschrift für Geburtskunde*, edited by von Busch, Ritgen, and Siebold, Berlin, 1852, Art. II. p. 313, has a paper by Dr. Von Busch, relative to an epidemic of childbed fever in the lying-in hospital of the University at Berlin.

In February and March, 1851, after an antecedent epidemic grippe, childbed fever became more extensively epidemic than it had been for many preceding years. It broke out among the women in the hospital in the month of February, the cases being of a mild type, amenable to treatment; but in March, it prevailed with great violence, so that soon, almost all the women were seized, and many of them died with the disease. In consequence, the hospital was closed in order to spare the mortality, and it was shut up during six entire weeks, and was, during the time, carefully cleansed and ventilated. Dr. Von Busch supposed that such a perfect cleansing and ventilation might relieve him from the pain of seeing so many women's lives exposed to danger by being confined in a house whose atmosphere had proved highly deleterious, yet he was sorely disappointed when, upon opening it again for admissions after the six weeks had elapsed, he observed that all the women who now came in were seized with the childbed fever with greater or less violence, on the day following their delivery. A similar incident is related as to the Dublin Hospital, for we find that Dr. Robert Collins, in his great work, at p. 387, says, that during the epidemic of 1819-20 in the Dublin Hospital, Dr. Labat, the master, used the greatest care in ventilating and cleansing the house, but could not check the progress of the cases. "In one instance, a ward, in which there had not been any patients for several months, and which in the mean time had been kept strictly clean and well aired, was opened, and five patients admitted, three of whom were seized with puerperal fever and died." This does not look like effects of contagion. It is a malaria rather; there was poison in the house.

155. Under the alarming circumstances existing within the lying-in institution at Berlin, Dr. von Busch was inspired with the happy idea of clearing the house of its pestilential atmosphere by means of an elevated temperature, and accordingly, he directed one or more stoves to be placed in each of the wards. The doors and windows being then all closed, fires were made of stone-coal in all the stoves, and the apartments were heated to 52° — 60° R., the heat being steadily maintained during two consecutive days.

156. The result was surprising, inasmuch as from that day forth, during the whole semester, not one single woman was attacked in that hospital, although childbed fever cases still continued to occur in the city. If, now, the disease was contagious in von Busch's hospital, could there be a more virulent one than that he described; or, if it could become contagious, was not this the most likely place in the world for it to acquire that character?

157. In the month of December, four persons in von Busch's hospital were attacked, of whom one died. Hereupon the ward was immediately vacated, and then heated as before, and with complete success, for from that time until now (June, 1852), not another case has occurred.

158. Do you say again, facts are stubborn things? I grant you. Well, then, here are very stubborn facts. Yet I know not what you may say or think about this most interesting relation by Dr. von Busch.

159. The celebrated Parisian Professor, Paul Dubois, who is, I believe, looked upon as the person now at the head of the list of individuals illustrious in this art in his native country, says, at p. 342, *Dict. de Méd.*, tome 26: "When, in an establishment (for lying-in women), several women are successively attacked, it is impossible to say whether there was infection or contagion in the case" * * * at p. 343, he proceeds: "Relatively to contagion there is a point still more controvertible. It is not repugnant, perhaps, to many physicians, to admit there may be contagion from one sick woman to another woman in good health. But what shall we say of an indirect contagion, of which a healthy person, in some way, serves as the vehicle, and which could not pass from one woman to another except in this way? The possibility of such transmissions has been suspected, inferring from facts worthy of attention,

doubtless, but the significance and importance of which have been singularly exaggerated, through ignorance, malice, or the spirit of system, the histories of them not being sufficiently circumstantial or precise to carry away the conviction of sober men; and which, besides the way in which they have been stated, admits of a much more natural interpretation."

160. Such are Prof. Dubois's words. You can judge of their meaning and value. Yet I should be unjust here, were I to fail in citing the next following sentence, as follows: "In a question so little understood, to doubt is at least a duty: nothing but numerous and impregnable proofs can establish in science, a fact relative to a mode of transmission, which is rejected, for the present, by the generally received opinions of pathological physiology and pathology." If you claim Paul Dubois as a contagionist, you may examine your title to do so in the foregoing. Yet Paul Dubois is one of the men most likely to be a contagionist, for he firmly asserts there is a puerperal fever.

161. If you must persist in asserting that the disease is a contagious one, you must admit that it should be most so in crowded wards, and you ought to infer, nay, know that von B.'s patients must have been poisoned, partly by the women sick there, and partly, at least, by the professor and his assistants, who killed these poor women by contact or *ad distans*; for, according to you, the nebulae or halones must have been partly wafted from bed to bed, and partly inoculated by the physician's hands. Even the beds and other furniture must have been full of contagion; as full, at least, as the persons and clothing of the physicians and nurses. Yet as soon as von Busch had baked the house in a heat of 52°—60° Reaumur, for two whole days, your contagious matter, as you call it, was utterly destroyed, eradicated, or driven away, and the poor women who next came in found themselves perfectly safe. The professor did not remove the contagious beds or any portion of the contagious furniture. On the contrary, he left the wards in the very condition they were in when the last patient was dismissed; and he did so, doubtless, with a view to subject, not the wards only, but all their furniture to the disinfecting power of heat at 52°—60° R. I may venture to say he baked the cause, and thereby deprived it of its noxious quality, just as we bake or cook certain articles of food, which, in their raw state, might poison us.

162. Those of you who are contagionists ought to contend that Prof. von Busch, and all his aids, nurses, and servants, should have been baked, for forty-eight hours, along with the walls and ceilings and furniture of the hospital; because, say you, the contagion, halo, or nebula would come back with the re-entering patients and doctors, after the three days should elapse. Do you not, you who are contagionists, believe; nay rather, do you not know, that this nebula must invest them like the invisible cloud about the form of Cytherea, when she walked along with the pious Æneas?

“At Venus obscuro gradientes aere sæpsit,
Et multo nebulae circum Dea fudit amictu,
Cernere ne quis eos, neu quis contingere posset,
Molirive moram, aut veniendi poscere causas.”

ÆNEID, Lib. i. 411.

163. If Professor von Busch, and his aids and servants, could not, according to your hypothesis, but represent what Dr. Oliver Wendell Holmes, in his *Contagion of Puerperal Fever*, calls a “private pestilence,” it is certainly a staggering answer that all these private pestilences should have been so completely disinfected by von Busch’s stoves; and I take it for granted that a mattress or a pillow, on which a woman has exhaled the contagion of child-bed fever along with her last dying sigh, ought to be as valid a “private pestilence” as a professor, student, nurse, or any ward-maid. The latter were not there.

164. I with great satisfaction here refer you to Dr. Robert Collins’s work, before cited, wherein, at p. 387, he recounts the triumph he obtained over the *cause* of childbed fever by purifying the whole hospital. In February, 1829, it was scourged with the epidemic. He turned out all but the most destitute of the women, filled the wards, in rotation, with chlorine gas, closing the windows for forty-eight hours during the disinfecting process. He painted the floor and all the woodwork, with chloride of lime mixed with water, to the consistence of cream, and left it on for forty-eight hours more. After this, the woodwork was painted, and the walls and ceilings whitewashed. The blankets, &c. were scoured and then stoved at a temperature of 120° to 130°. From this time, February, 1829, until the close of his mastership, in November, 1833, Dr. Collins lost not a single patient with the disease in Dublin Hos-

pital, a result highly honorable to the distinguished physieian and philanthropist. Where was the contagion fled? Dr. Collins and his assistants, matrons, and ward-maids were not chlorinized; were they ever, indeed, private pestilenees?

165. Dr. Holmes would not be understood to imply that there exists a doubt in the mind of any "well-informed member of the Medical profession" as to the fact that puerperal fever is "sometimes" communicated from one person to another, "both directly and indireetly." Yet Prof. Seanzoni, who is a well-informed man, perhaps even as well-informed as those I have already eited for you, in his *Lehrbuch der Geburtshulfe*, Band III., ii. Abtheil. p. 468, says: "With these views is connected the question whether puerperal fever is eontagious or no. There are still to be found some physicians who suppose that this question must be answered affirmatively. We, for our part, are of the opposite opinion, because, out of our own eonsiderable experience, there has not appeared a single ease in which we could or ought to make out a contagious origin. All the grounds presented by the contagionists in support of their hypothesis are either without proof, or refer only to the existenee of a miasmatic or epidemic causation of the disorder; or finally, lead to the inferenee of some deleterious substance, matter, poison, &c., &c., brought from a sick lying-in woman within the economy of a sound, healthy woman, and that gives rise to a blood disease, while, certainly no man can speak here as to any specific contagion."

166. It is true that Seanzoni afterwards refers to Skoda and Semmelweiss's opinion, that some of the cases in the Vienna Hospital were communicated by the hands of persons who had recently assisted in dissections; and he adds, relative to the sentiments of Kiwisch, Seyfert, Lumpe, and Zipfl, that it would occupy him too long to trace out their views against contagion in the cases, and refers to their admirable writings only for the purpose of remarking "that he will not deny the possibility of an infection in certain cases, but considers it is going too far, to attribute the puerperal attaeks in hospitals to this principle of contagion alone."

167. In this last passage, while Seanzoni admits, or rather will not deny, that the disease is, in certain cases, communicable, I do not eoneeive he allows of its eontagiousness; but only, that disease,

being developed in a lying-in woman, that disease is apt to assume the nature of childbed fever. As far as that I am willing to go myself; for I am aware that anything that could set on foot a diseased train in such a person, could scarcely fail to develop childbed fever. This, however, is far from the idea of a contagion *à contactu*, or *ad distans*.

168. It may be presumed that no person now denies that dissection wounds are very dangerous; and every well-informed anatomist, about to make a *post-mortem* examination of the putrefying viscera, after a fatal peritonitis, or any other disease, would be extremely careful not to interfere with such a subject should he have, at the time, any abrasions of the fingers or hands. See the cases of Mr. Wm. Hutchinson, and Mr. Dease, as they are stated by Mr. Colles, in *Dublin Hospital Reports*, vol. iii., p. 204 to 216. I should think that no experienced accoucheur could read these two cases of dissection wounds, without finding in them evidences to show that the constitutional disorders, growing out of the accidents, were due to a phlebitic disorder, or, to employ a better word, to an extensive inflammation of the Endangium. To wound the fingers in the delivery by embryotomy, of a long decomposing and offensive foetus, by spiculæ from the cranium, or by the point of a sharp crotchet, is a very dangerous accident, and is followed by great distress, as I have more than once experienced in my own person. But to carry the putrid material substances of a dead body, and to inoculate with them the organs of a woman in labor, even though fatal, ought not to induce you to consider such a childbed fever as contagious. I beg you to read the shocking hystoirette, at p. 356 of *An Introduction to the Study and Practice of Midwifery, and the Diseases of Women and Children*, by Dr. Wm. Campbell; London, 1833; 8vo. The author's words are: "In the autumn of 1821, he attended the dissection of a married woman, who died of the disease, after an abortion of the early months; removed the pelvic viscera and external parts, and carried the whole, in his coat pocket, to the class-room. The next morning, *dressed in the same clothes*, he assisted, with some of his pupils, in an instrumental delivery at Bridewell. This woman was seized with the same affection, and died. The same night, he accompanied Dr. Orr to the delivery of a woman residing in the North-Back of the Cannongate; she was

equally unfortunate, and three other poor women shared the same fate in quick succession." In a subsequent year, 1823, he assisted at the dissection of a childbed fever case, but "did not wash his hands with the care he desired." Thence he went immediately to attend two other women in labor, both of whom died. Do you remember that Bichat died for looking into a jar in which he had an anatomical specimen macerating, and that Prof. J. S. Dorsey, of the University of Pennsylvania, lost his life from a like cause? No man ought to carry home viscera in his pocket, and wait on sick women in the same clothes. This I admit most cheerfully. Dr. C., however, did not long continue to be a "private pestilence." I repeat, that in epidemic seasons the cause of childbed fever exercises a sort of dictatorship over women; and whether you bring on fever by fatigue, by burns, by cold, exposure, improper diet, or by whatever cause, including this inoculation, it becomes this very childbed fever—it could become nothing else. If the disorder arises from a laceration of the uterus, it is no more true to say of it, it is contagious, than when it arises from a dissection virus. Such dissection virus may kill a man or a virgin, as well as a lying-in woman, but it could not give either of them a childbed fever. Is not this a reasonable and just discrimination? Does it leave any ground on which to found the belief that a man may carry a nebula for weeks about his person, or radiate, *ad distans*, so virulent a poison?

169. I have practised midwifery for many long years; I have attended some thousands of women in labor, and passed through repeated epidemics of childbed fever, both in town and in hospital. After all this experience, however, I do not, upon careful reflection and self-examination, find the least reason to suppose I have ever conveyed the disease from place to place, in any single instance. Yet for many years I carefully considered whether such transfer, by a third person, might be possible, and carefully read the statements of various authors to that effect. In the course of my professional life I have made many necroscopic researches of childbed fever, but did never suspend my ministry as accoucheur on that account. Still, I certainly was never the medium of its transmission.

170. I have, in numerous instances, gone from the bedside of

women dying with childbed fever, whether sporadic, or to the most malignant degree epidemic, without making my patients sick. I have also endeavored to assist my brethren, when they had such cases and I had none.

171. I did not then, nor should I now, fear to be the medium of transmission. But I have been unceremoniously set aside, after having been for months engaged, even for some who owed me repayable gratitude for the services I had for years rendered them. And this treatment I got, not because I merited it, for I did not merit to be regarded as a private pestilence, nor was I found to be so, in fact, by those who had more good sense, or who could appreciate the feelings with which a physician finds himself to be looked upon as a peripatetic pestilence, or poisoner of women for love of gain, or what is worse, stupidity.

172. In a series of labors, 468 in number, and beginning with No. 1, I find that Nos. 18 and 19 were affected, and that No. 18 died with childbed fever; No. 31 was sick, but recovered; Nos. 195 and 259 were sick and recovered; but No. 291 died, as did also 293. Nos. 332, 339, 435, 444, 445, and 455 were attacked and recovered. The above cases, viz: 18, 19, 31, 195, 259, 291, 293, 332, 339, 435, 444, 445, 455, are in all 13 cases in 468 labors, of which three died and nine recovered. Now if I was the medium of contagion for any one of that series of 468 confinements, why did I poison them in the ratio and order above set forth; and why did I not communicate the disease in more than 13 out of 468 cases? What became of my nebula from 31 to 195; to 259, and between 291 and 445, and so to the end, or 468? Such a table is far more easily explained by regarding the falling out of the cases as coincidences and accidents, than as material causations through a "private pestilence."

173. At page 631 of my work on *Obstetrics*, 2d edition, I have related the circumstances attending the practice of a physician of Philadelphia, who, in one of our epidemic seasons, lost a considerable number of women in childbed. I beg to refer you to that page, where you will see how he lost one of the number, whom he did not visit until she was advanced so far in the disorder, that upon the first inspection he pronounced her case hopeless. This case swelled his list equally with the first one he saw, to which it is not possible that he should have communicated the poison.

His patients were scattered over a great superficies of the city and districts, some of them being more than two miles from others. At that time, many women were attacked, in various parts of Philadelphia, as well as in the State of Pennsylvania; yet, so far as has come to my knowledge, no other medical gentleman happened to encounter such a great number of childbed fever cases as he did. I visited in consultation with him some of the very worst of the cases, and touched the patients, and was as liable to imbibe, or to be clothed with the effluvia from their bodies as he was; nevertheless, I did not carry any poison, or other cause of disease, to any patient of mine; and if not I, then how should he become capable of doing so? He is a gentleman who is scrupulously careful of his personal appearance, of great experience as a practitioner, and well informed as to modern opinions on the contagion of childbed fever. Still, those of you who are contagionists will say that he carried the poison from house to house; and if so, then you ought to give some rationale of the fact. Did he carry it on his hands? But a gentleman's hands are clean. Did he carry a nebula or halo about him? Then why not I also? If the nebula adhered to his clothing, it might as well have adhered to mine.

174. What will you say, young gentlemen, of the experience of my friend, Dr. D. Rutter, formerly of Philadelphia, but now of the city of Chicago, who passed through terrible scenes here in an epidemic of childbed fever some years ago, when he had a most extensive midwifery practice in town and country—I pray you, refer to my *Letters to the Class*, 3d edit. p. 591, for what I have there related on this subject. During that sad time, I saw several fatal cases with him, in consultation; and, though he seemed to be tracked by the cause of the disease, to judge by the numerous attacks of it in his lying-in patients, I was not tracked by it; I took no precaution, except such as every decent man should be supposed always to take; yet I never did carry the disease from his cases to any house where I visited lying-in women. But he was charged with being a carrier of contagion. How could he carry the cause? What was the cause—was it some oozone that stuck to his hands or coat? Was it a nebula, a halo, or an effluvia, or a miasm that mixed with the hairs of his head, or the woollen or cotton fibres of his dress; or an exhalation from his

skin, or a halitus from his lungs, like the fiery breath of Cacus? and can you say of him, as Virgil sings?—

“Faucibus ingentem fumum, mirabile dictu,
Evomit?”

ÆNEID, Lib. viii. p. 252.

175. Come, now; was not such a poison more sticky than bird-lime, seeing that Dr. Rutter, worn out with fatigue, and wounded in spirit by his cares for the unfortunate victims of an epidemic disease, left the city for the purpose of regaining some strength, and to escape from the repetition of such disheartening labors, and that even a quarantine could not liberate him from this poisoned cloud! One might hope it would have been blown away by the wind, or that it would have evaporated, or become too dilute to kill, after a ride of seventy miles, and an absence of ten days! But it happened, after this rustication of ten days, at a distance of thirty-five miles from the city, that your birdlime, or cloud, still adhered to him, as you contagionists would say. And more than that, he could not even wash it away, or shave it off; for, upon coming back to the city and to his professional toil, before he engaged in practice again, he caused his head to be close shaved; he entered a warm bath, and washed himself clean; he procured a new wig, new clothes, new hat, new gloves, and new boots. He did not touch anything he had worn, and took the precaution even to leave his pencil at home, and his watch. Well, what do you think happened next? He went out to attend a lady in labor, who had a favorable parturition; yet was next day assailed by a horrible childbed fever, of which she died, in spite of all his efforts, and mine, to help him; for he called me in consultation immediately after being himself summoned to her chamber. I know that that lady died with peritonitis. I was a great deal with her in the illness; but she did not poison me, nor my clothes; for, though I went on with my practice, I poisoned nobody, and made nobody have even so much as a finger ache!

176. Dr. Rutter repeated this attempt at personal disinfection at a subsequent period, which was two years later, and with the same ill success. The gentleman was much and disparagingly spoken of on account of the above-mentioned events in his practice, which I cannot but regard as both cruel and unjust, particularly as his

success in the treatment was most brilliant; for during the epidemic he had charge of seventy cases, of which he lost only eighteen, and I know not the man who can boast of a higher triumph of his art of healing in this malady.

One of the most decided of contagionists was Dr. Douglas, of Dublin. He carried his doctrine very far, for he supposes that the contagion is so virulent that it may at times affect even virgins. His paper was published in *Dublin Hosp. Reports*, vol. iii. p. 144, and you may read in his "Report" that, "when puerperal fever is epidemic, I consider it really contagious, but, for the most part, only to lying-in women;" yet he thinks that women pregnant or nursing, and even some who have been several months confined, and even unmarried women, "at particular periods," might be liable.

177. That estimable physician, the late Dr. Goode, of London, whose character and talents I shall never cease to admire, was a most firm believer in the contagion of puerperal fever, as you shall find by reading his *Account of some of the most important Diseases of Women*. Though an admirable writer, a most learned man, and judicious practitioner, he appears to me to have looked into this subject with a mind so prejudiced, that anything and everything coming in his way to illustrate or enforce it was seized upon with an avidity more suitable to a *globe-mouche* than to a person of his intellectual rank. At p. 75, speaking of the frequent occurrence of cases in the practice of particular individuals, he says: "One instance of this kind was very remarkable; a general practitioner, in large midwifery practice, lost so many patients from puerperal fever, that he determined to deliver no more for some time, but that his partner should attend in his place. This plan was pursued for one month, during which not a case of the disease occurred in their practice. The elder practitioner being then sufficiently recovered, returned to his practice; but the first patient he attended was attacked by the disease and died. A physician who met him in consultation soon afterwards, about a case of a different kind, and who knew nothing of his misfortune, asked him if puerperal fever was at all prevalent in his neighborhood; on which he burst into tears, and related the above circumstances." Now it is evident that good Dr. Goode states this incident as confirmatory of his views concerning contagion; and to

render the inference the stronger, gives the touching picture of the physician's grief; but surely the poor man's tears do not prove the contagion to have been carried by him, after a quarantine of thirty days. Or does Dr. Gooch hint that the "general practitioner" was himself a generator of the poison? If he was not a diseased man, and capable of generating the miasm in his own economy, what was it that he had "sufficiently recovered" from? Had he recovered of his nebula, his halo, his *ingentem fumum*? Men are said sometimes to be under a cloud; but I know not how a man is to recover from his childbed fever cloud or *nimbus*.

178. *Facts and Observations* relative to the fever called *Puerperal*, by John Armstrong, M.D., is a work of great merit and name. This gentleman saw the epidemic cases at Sunderland, in the year 1813. At p. 171 (Barrington and Haswell's edition), he refers to Gordon's sentiments, and proceeds: "And I am now well convinced that when puerperal fever is once generated, there is almost always cause to apprehend its being communicated to other puerperal women, especially by accoucheurs and nurses who have previously waited on affected persons." Dr. Armstrong does not argue the question, but gives us purely and simply his own belief. I request for Dr. Armstrong's opinion your most respectful deference; but I at the same time beg you to remember, that there is a wide difference between belief and knowledge. There is great risk, in studying any book written by such a man as Dr. Armstrong, that you should take his opinions for facts, and believe merely because he believed thus or so.

Professor Jörg (sec. 747, p. 702 of his *Treatise on Diseases of Women*) says of epidemic childbed fever, that it prevails with greatest virulency in crowded hospitals, breaking out without any of the causes he had before been explaining. Contagion, according to him, occurs only under such circumstances as these, to maintain the disorder, and the best and strongest health affords no security against its attacks.

I advise you to read *A Treatise on Puerperal Fever, &c.*, by Wm. Hey, Esq. This gentleman saw the epidemic cases at Leeds from 1809 to 1812. He was a very careful and conscientious writer, and judicious practitioner, who, being fully aware of prevailing opinions on its contagiousness, gave his attention to the evidences arising in his own practice. Dr. Hey is certainly to be classed among those who reject the doctrine.

179. Having not the least wish to persuade you to be of my opinion, by any other means than those connected with expositions of truth on the subject, I ought, while defending my own sentiments, to show you the opinions of others of an opposite kind. Among the most considerable advocates of contagion was Dr. John Roberton, of Manchester, England, author of *Essays and Notes on the Physiology and Diseases of Women, and on Practical Midwifery*, Lond. 1851. This writer, at page 439, tells us that the Medical officers of the Lying-in charity of Manchester met, January 4, 1851, in consequence of a great mortality in the preceding four weeks, in the practice of a certain woman, who was one of the twenty-five midwives belonging to the establishment. That Charity usually had in charge about three hundred and eighty labors per month, or four thousand five hundred and ninety per annum.

180. On the 4th of December, 1850, the said midwife, Mrs. *x*, delivered a woman, who soon afterwards died; and this is the first fact in the statement of the history of the epidemic; and it should be observed that nothing is said in relation to the cause of the first woman's attack. Mrs. *x*'s books, upon examination, showed that she had delivered in

1850, December 4,	1	woman:	Had	puerperal	fever,	1
"	"	5, 1	"	"	"	0
"	"	6, 2	"	"	"	1
"	"	7, 4	"	"	"	1
"	"	18, 3	"	"	"	2
"	"	22, 1	"	"	"	0
"	"	23, 1	"	"	"	1
"	"	24, 1	"	"	"	1
"	"	25, 2	"	"	"	2
"	"	26, 2	"	"	"	0
"	"	28, 1	"	"	"	1
"	"	30, 2	"	"	"	1
"	"	31, 1	"	"	"	0
1851, January 1,	4	"	"	"	"	2
"	"	2, 2	"	"	"	1
"	"	3, 2	"	"	"	2
<hr/>						
Labors	30		Cases	16		

181. All these cases of childbed fever, sixteen in number, more than half of all the women delivered by Mrs. *x*, proved fatal; which excited the feeling of alarm, and led to the meeting of the officers. The other twenty-four midwives of the Charity, who were continually crossing Mrs. *x* in their routes of practice, met not with a single case, though about ninety women per week were brought to bed by them. Dr. Robertson says: "The fact that sixteen fatal cases of puerperal fever occurred in one month, in the practice of a single midwife, while the patients of the other midwives were exempt from attacks of the disease, leads to the conclusion that the midwife was the means of communication (I take not upon myself to say in what manner) from one woman to another, from one affected with the fever to another in health."

The author proceeds to say "That, besides being infectious, that is, capable of being conveyed in some tangible medium from one woman to another, this disease is propagated by a cause of a more general kind, probably existing in the atmosphere after fever has prevailed for some time in a locality, cannot be doubted."

Hear him again: "A short time after the meeting (January 4), the puerperal fever, among the patients of the other midwives as well as in private practice, began to appear in various parts of the town. It never prevailed more generally, nor perhaps more fatally in Manchester. But about the beginning of June it had disappeared."

182. These accounts, contained in Dr. Robertson's book, have been greatly relied upon as not evidence merely, but as proof, that the disease was transmitted by the unhappy midwife, Mrs. *x*; and one ought to infer that if the accusation against her as a peripatetic pestilence, or "private pestilence," could be made good, the whole epidemic, with all its terrors and fatalities, might be charged upon that miserable woman, as fruits of her power to poison through her Cacus-breath, her steeped hands, or her halo of miasm.

183. Dr. Robertson enjoys an enviable reputation as physician and writer, and I hope that there are few persons more ready than I to acknowledge his great merits as a member of our Class. Yet I cannot escape the conviction that all this relation concerning the Charity midwife is an unintentional wresting or distortion of the

pathogenic truth of the matter. Is it improbable, to say the least, that he had no better reason to accuse Mrs. *x* of poisoning the third than the first patient, of December 4, 1850? She could not have poisoned No. 1, because she was not yet herself poisoned. She did not poison No. 2, nor No. 7, nor No. 12, nor No. 13, nor No. 15. If it was the fault of Mrs. *x* to establish the infection "in a locality," so as to give it power to be contagious when "the fever had prevailed for some time in a locality," then Mrs. *x* was the real dispenser of the cause of the Manchester epidemic; for if that poor woman had not been in practice, the first sixteen cases would not have appeared. She spread abroad the contagion until it became contagious. Mrs. *x* was deemed so dangerous that she was suspended of her functions and sent out of town. Yet about the first week in January (there had been sixteen cases) it had got among all the people, even in private practice. What a very concentrated nebula (*multo amictu*) must it have been, that clung around the poor woman's farthingale!

184. It is much to be regretted that Dr. Robertson did not give us some clear notions relative to the amount of the fatalities of this bad epidemic, instead of merely saying it "never prevailed" more extensively or fatally in Manchester; for we should be better able to come to a conclusion as to contagion, or no contagion, if we could know the number of the victims and the time required to destroy them.

185. The *Ninth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England*, shows, p. 74, that in 1846 there were born in Manchester 8,279 children, which may be assumed as near the number for 1851. Now, as the twenty-five midwives of the Charity had about 380 labors per month, they would have 4,560 labors in a whole year. Mrs. *x* lost sixteen women in one month. The epidemic lasted for six months, and was never more extensive and "fatal in Manchester." It had got, by the first days of January, among the other twenty-four midwives, and even into the walks of private practice. There must have been born 2,280 children in the six months of its reign, whose mothers should have been exposed to the virulent poison! How many of these did it really affect? This question cannot be answered. Yet if we take Mrs. *x*'s results as the basis, we may conclude

that many hundreds of women were seized and destroyed; for, as she lost sixteen out of thirty cases, the other twenty-four women may have had an equal number, which would give us 384 deaths in that epidemic. I trust, however, that not above one hundred lying-in women died.

186. If the whole number of births at Manchester was 8,279 per annum, there were born in the epidemic reign 4,189 children; and if, of all those mothers, only one hundred died with childbed fever, or even if 384 of them perished with it in a vast and crowded population, many of them miserably poor and ill fed, how hard must it be to prove that contagion had anything to do with the matter? How hard a thing it is to come to such a result, after a dispassionate inquiry, I now leave you to judge.

187. The vol. of the *Syd. Soc.*, 1849, contains Dr. Fleetwood Churchill's *Historical Sketch*, at p. 41 of which, that gentleman writes, that "The evidence and proofs thus adduced, are of extreme importance; and I fear we must conclude, however reluctantly, in favor, not merely of the contagiousness of puerperal fever, but of the possibility of its contagion being carried by an intermediate party. This makes the practice of midwifery doubly distressing during the prevalence of an epidemic, and ought deeply to impress us with the necessity of the utmost care and caution."

188. Dr. Fleetwood Churchill has referred to Alex. Gordon's cases; to Gooch's instances; to the cases at Sunderland, where 40 out of 53 persons came into the care of one single surgeon; to Mr. Robertson's examples, as above; Dr. Pierson of Salem; to Dr. Condie's belief; and to Peddie, Beecroft, Copland, and Campbell. As all these instances put together do not confirm, or even strengthen the instance given of Dr. Robertson's nurse, Mrs. x, I shall not load my book with them. You can, if you please, explore these evidences, called by Dr. Churchill proofs, at your leisure, and found your matured opinions upon the fullest and . . . most searching inquiry. I will make, however, one remark in connection with what Dr. Churchill, and others, have said about the "utmost care and caution," and it is this: If obstetricians do, in fact, carry the poison of childbed fever with them, there is really, it appears to me, only one step for them to take, and that

is, to abandon the study and practice of Midwifery altogether. It is idle to make a rule, as some scribblers propose, of desisting from practice for a week, or a month. There can be no use in a quarantine, when the law of the contagion is wholly unknown; and if a physician is to have 12 cases per annum, one breaking out on the first day of each month, to quarantine him for 30 days each, he will be a person of but little use to the community as a practising accoucheur. It would be better to quit at once and forever, and leave the women to take care of themselves, or refer their wants to the Shiphrahs and Puahs, who, after all, might haply turn out to be veritable Mrs. x's, or gentlemen-pestilences, and so develop an epidemic as fatal and general as that of Manchester in 1851.

189. Most assuredly, could I but once bring myself to believe in the contagion of childbed fever, I could never afterwards be induced to practise my Art. All the malignant assaults on us in the volume of Elizabeth Nihell, and all the objurgations uttered against us by the good old priest, spoken of by Dionis, *Traité générale des Acc.*, Paris, 1724, p. 427, could not so much influence my conduct, in this regard, as a momentary doubt of the transferability of this mortal disease. I utterly reject and deny it; and, of course, I shall not be distressed because two very young gentlemen say, "We think the contagiousness," or, to use the more explicit term used by Dr. Christison, the "communicability" "of childbed fever, in all its forms, is a fact established on the most irrefragable evidence; so much so, that it would now be almost criminal for any practitioner to act on the opposite assumption." *Vide* McClintock and Hardy, *Pract. Observations*, p. 29. To all which I have only to add, *Voilà comme l'on écrit l'histoire!* What is the precise difference between being "almost criminal," and very criminal indeed?

190. But alas! gentlemen, nurses are as fell in their poisoning qualifications and nature, and are as prone to become "private pestilences" as we abused physicians; and as the women will require some aids, some skill, some science in their many troubles, what are they to do? Twenty-five centuries have hardly served to lift our Obstetrics from the depths of barbarism and ignorance, into that somewhat dawning light it now enjoys, as I have

already shown in these Letters. And shall we now go back again to the capabilities of a Celsus, or an Avicenna, or an Avenzoar? Or shall we rather disregard the jejune and fizenless dreamings of sophomore writers, who thunder forth denunciations, and would mark, if they might, with a black and ineffaceable spot, the hard-won reputation of every physician, who, in the Providence of God, is called upon to contend with the rage of one of the most destructive of epidemics, and pay an ungrateful service; indispensable to the victims it is allowed to attack, and in the propagation of which they have no more to do, than with the propagation of cholera from Jessore to San Francisco, and from Mauritius to St. Petersburg.

191. Let us then decide—I have long ago decided for myself to go on. Will you go on, or will you stop here? Is contagion a truth? Then, for heaven's sweet sake, I implore you not to lay your impoisoned hands upon her who is committed to your science and skill and charitable goodness, only for her safety and comfort, and not that you should, after collecting fees, soon return her to her friends a putrid corpse. What a horrid idea!

C. D. M.

LETTER VII.

CHILDBED FEVER, SPORADIC AND ACCIDENTAL, OR EPIDEMIC AND UNAVOIDABLE—TABLE OF EPIDEMICS FROM 1652 TO 1853—IT IS NOT A FEVER, BUT A PHLEGMASIA—FEVER, WHAT IS IT?—A GROUP OF VARIOUS INFLAMMATIONS, DIFFERING IN CHARACTER—OPINIONS OF VARIOUS AUTHORS AS TO THE CAUSE—DR. FERGUSON, CHURCHILL, DANCE, TONNELLÉ, CH. WHITE, TONNELLÉ'S DISSECTIONS—CASE BY AUTHOR—GORDON'S WORK—CASE BY AUTHOR—REMARKS ON THE SO-CALLED VITIATION OF THE BLOOD.

192. CHILDBED fever, which is always one and the same, to wit, a phlegmasia, may occur either as a sporadic or as an epidemic disease. For example, a woman in labor may suffer some contusion or laceration of important parts; or she may have suffered, to a great degree, from fatigue, or terror; or she may be affected by cold damp, or by improper diet or drinks; or she may have been previously sick in such a way as, after labor, to develop within her frame that train of morbid conditions which constitutes a childbed fever. Such an event occurring now and then, but rarely, would be denominated a sporadic childbed fever, or puerperal fever. There is an almost infinite number of contingencies in the state of a woman's general health, that might, after the completion of her labor, cause the rise, and insure the rapid destructive progress of inflammation, either in the organs of generation, or, in the serous lining of them, within the pelvis. Any of them, thus acting, might develop a childbed fever, which you would characterize as a sporadic case of the disorder. It is admitted that many towns, cities, districts, and even whole countries are observed to present, for a series of years, only rarely,

cases of such sporadic childbed fever; and the causes of the attacks, in such instances, being for the most part easily to be ascertained, the cases may be considered as accidental cases.

193. On the other hand, it shall happen that women, confined in any town, city, district, or country, shall be observed to be, very generally, assailed by a disease, whose physical characteristics do not materially differ from those of the sporadic or accidental cases, just mentioned; and when we come to inquire into the causes of these numerous, or almost universal attacks, we can find none palpable, and so are obliged to fall back on what we think or know of contagion, infection, malaria, or epidemy, in order to satisfy our cravings after knowledge or explanation. Such cases are cases of epidemic childbed fever, and, as such, ought not to be looked on as accidental, like the first-named class, but rather as unavoidable cases.

194. It is a very easy thing for a man to understand how it may happen, that puerperal women should be attacked with destructive inflammation in the pelvic and abdominal viscera, in consequence of the different lesions I just now mentioned; and one would naturally expect that a violent endo-metritis, or a rapid uterine phlebitis, or an extensive peritoneal inflammation, should soon place the woman's life in peril, or even hurry her, after a very short course of such inflammation, to the grave; because the lying-in state is one that readily exposes to the assault of inflammation. You would not, therefore, feel greatly surprised to find, now and then, that a person, whom you had attended in labor, should be seized with and die from the effects of such an inflammation; and such instances must be met with by every person considerably employed in the practice of Midwifery; being accidents that must, and will attend the trials of parturition. They have been observed in all ages and nations, and must be met with hereafter, as long as the nature of woman and the physiological laws of parturition remain unchanged.

195. But, when the disorder becomes epidemical, seizing upon a considerable part, or all the women who happen to be brought to bed within its reign in a certain village, or hospital, or country, then the mind becomes astonished and bewildered, and hastens to lay hold upon any probable or possible rationale of events so

distressing that they not only render the practitioner himself unhappy, but excite a great inquietude throughout the largest communities.

196. Childbed fever becoming epidemical may prevail so extensively as to implicate almost all the women who are brought to bed under its reign; or the force of the cause may be so slight as to produce illness in only here and there an unfrequent example, so that the epidemic cases may be very rife or not. Hence it will occasionally be a difficult thing for you to say whether the cases you meet with should be called sporadic or epidemic ones; and the means by which you will at length determine the point, will have relation to the frequency rather than to the phenomena of the attacks. For, unless I greatly misinterpret both history and my own experience, there is little difference in the manifestations in the sporadic and epidemic forms. Under certain circumstances, however, you will find the epidemic cases more difficult to cure than the accidental ones. Dr. J. C. Douglass, in *Dublin Hospital Reports*, vol. iii. p. 142, thinks that not more than one in six of sporadic cases will be found to prove fatal, "whereas, in the truly epidemic fever, hardly one-half of those absolutely attacked recover." But I must defer my remarks, on this point, to a later page.

197. Ozanam, in his history of epidemics, gives us an account of different dates and places where this malady has been observed; so likewise does Dr. Meissner; and F. Churchill, in his *Midwifery*, has made out a table of the instances. From these different sources I have arranged the subjoined table:—

What I have derived from Ozanam is marked with the letter O; from Meissner, with the letter M; from Churchill, with the letters Ch.

TABLE OF EPIDEMIC OCCURRENCE OF CHILDBED FEVER.

A. D.	PLACE OF PUBLICATION.	AUTHOR.	INITIAL.
1652.	Leipsic.	Welch.	O.
1664.	Paris.	Peu.	Ch.
1672.	Copenhagen.	T. Bartholin.	O.
1723.	Frankfort and Leipsic.	F. Hoffman.	O.
1746.	Paris.	De Jussieu, &c.	O.

A. D.	PLACE OF PUBLICATION.	AUTHOR.	INITIAL.
1746.	London.	J. Clark.	M.
1750.	Lyons, Paris.	Doublet, Pouteau.	Ch.
1760.	Aberdeen.	Gordon.	Ch.
1760.	London.	White.	Ch.
1764.	Dublin.	J. Clarke.	M.
1765.	London, Derbyshire.	Moore, Butter.	M.
1767.	Normandy and Dublin.	J. Clarke.	M. Ch.
1769.	London.	Leake.	O.
1770.	London.	Leake.	Ch.
1770.	Vienna.	Fauken.	O.
1771.	London.	White.	Ch.
1773.	Edinburgh.	Young.	Ch.
1774.	Paris.	Doulcet.	O.
1774—1781.	Paris.	Doulcet, Fenon.	Ch. O.
1774.	London, Dublin.	Moore, Collins.	M. Ch.
1776.	Vienna.	Stoll and Finke.	O.
1782.	Paris.	Doulcet.	Ch.
1783.	London.	Osborn.	Ch.
1786.	Lombardy.	Cerri, Tenon.	O. Ch.
1787.	Gottingen, Dublin.	Osiander, Collins.	M. Ch.
1788.	London, Dublin.	J. Clarke, Collins.	Ch. M.
1789—91.	Aberdeen.	Gordon.	Ch.
1792.	Copenhagen.	Rink.	M.
1795.	Vienna.	Jæger.	Ch.
1803—13.	Dublin.	Collins, Douglass.	Ch.
1808.	Barnsley.	Hey, Naegellé.	Ch. M.
1812—13.	Leeds, Sunderland.	Hey, Armstrong.	Ch.
1814—15.	Edinburgh.	Hamiltou.	Ch.
1814—15.	Northumberland, Pa.	S. Jackson.	M.
1815.	Munich.	F. Boyer.	M.
1817—18.	Pennsylvania.	Deweese.	Ch.
1818.	Wurtsburg.	Schloss.	M.
1818—19.	Bamburg, Dresden, Munich, Prague, Vienna.	Eisenmann.	
1818—23.	Dublin.	Collins.	Ch.
1821—22.	Glasgow, Sterling.	Campbell.	Ch.
1823.	Vienna.	Leppich.	M.
1825.	Hanover.	Dommes.	M.
1826.	Dublin, Berlin.	Collins, Neumann.	M.
1827.	Barmen, Neuenhaus, Sunderland.		M.
1828—29.	London.	Gooch, Ferguson.	Ch.

A. D.	PLACE OF PUBLICATION.	AUTHOR.	INITIAL.
1829.	Paris Maternité.	Tonnellé.	Ch.
1830.	Giessen.	Ritgen.	M.
1833.	Vienna.	Bartsch.	Ch.
1833.	Prague.	Quadrat.	M.
1834.	Paris.	Bouchut.	M.
1835.	Vienna, Dublin.	Klein, Beatty.	M. Ch.
1838.	Stockholm, Paris.	Elliot, Willmer.	M.
1839.	Wangen.	Zengerle.	M.
1843.	Paris.	Bidault, Arnaut.	M.
1845.	Dublin.	McClintock.	M.
1853.	Philadelphia.		C. D. M.

198. Looking at this table, the question will properly present itself as to whether the examples set forth in it should be regarded as instances of epidemic prevalence of childbed fever, or whether you should consider the occurrences as examples of endemic disease; for you may prefer to discriminate between epidemic and endemic disorders.

199. It does not appear to me to be of any great importance which of the conclusions you should, under the circumstances adopt, for it is quite clear that the malady may break out, and rage with violence in certain circumscribed spaces; and on the other hand, that it may prevail, at one and the same time, over extensive districts, and even whole nations and countries, and yet be ever one and the same disorder.

200. The disease, wherever it is met with, can be only one, and the causes, whether endemic or epidemic, are, in all probability, the same; since, even in the accidental or truly sporadic cases, the phenomena are often not different, even in violence, from those of the most violent epidemic kind; and I consider that a woman dying of childbed fever from some accidental cause, as a contusion, laceration, or chronic disease of an ovary or a Fallopian tube, or the bladder of urine, passes through the same phases of morbid action, as she who dies in an empested hospital; since she gives out, during the progress of the malady, the very same manifestations, both as to the parts directly affected, and the affections of the whole constitution.

201. The object of making such a table as the above, being

merely to show, at one glance, that our disorder may prevail as an epidemic in different years, and at different and distinct places at the same time, there is no motive, perhaps, to enlarge it, which it would be very easy to do, especially from the records in the United States, where it has, for a considerable number of years past, committed ravages in Vermont, New York, Pennsylvania, Illinois, and Virginia.

202. I have heard of one small town in a northwestern State, where, out of some 600 inhabitants, about 60 parturient women died with it in a single epidemic. Dr. Ozanam, in his *Malad. Epidémiques Contagieuses et Epizo.*, tom. ii. p. 280, quotes Dr. Cerri, of Milan, to show that about the date of 1786, and the beginning of 1787, an epidemic broke out in Arzaga, in Lombardy, among lying-in women, in which none were spared. Dr. Wm. Hunter, of London, had charge of a hospital in which, out of 32 women attacked, 31 died; so that you are to understand that the epidemic cause, whatever it may be, is sometimes feeble, and at others most comprehensive as well as most devastating; and yet that cause, strange to say, acts only on pregnant and confined women; doing no harm to all other women, or to children or men!

203. But it is time now for us to begin to inquire more solicitously into the nature of the disorder; what it is, what are its seats, causes, signs, tendencies, treatment, differences; and what the prophylactic measures best adapted to the management of it.

204. In a former letter (V.), I at great length exposed for you the opinions of writers on the nature of the disease, and I now refer you to those sections, for what I have there said.

It was not until near the close of the last century, that our people began to open their eyes to the stark-staring truths revealed by numerous *post-mortem* examinations of victims to the disorder; for, as I showed you in sections 54, 59, 71, 75, in the last century they could not, or would not see the proofs of an inflammation, but only the signs of *lait répandu*, in the effusions and adhesions left in the belly by a destructive case; and the fact is, that, whenever they did so, they were obliged to come back again to the ancient admission of Mercado, and others before him, that childbed fever is an inflammation of parts contained within the abdomen

and pelvis, for that was the view taken of it by the old writers whom I cited for you in my second letter, sections 21, 24.

205. De la Roche, who wrote an esteemed book on the subject, printed in 1783, entitled it *Récherches sur la Nature et le Traitement de la Fièvre Puerpérale, ou Inflammation d'Entraîlles des Femmes Enceintes*; and he says, in his second page, "Cette maladie, à laquelle j'ai donné le nom d'inflammation d'entrailles, par des raisons qui seront développées ci-après, a été nommée par les Auteurs Anglais, Fièvre puerpérale, et regardée, par la plus part d'entr'eux, comme une espèce particulière de Fièvre putride." But you see here that De la Roche took a better view of the disorder, and, though he did not go directly to the point of believing it, with Paul of Ægina, to be inflammation of the womb, but inflammation of the bowels, he was much nearer the truth than Doublet, Puzos, Vigarous, Selle, and the whole host of the milkmen.

206. Though the discoveries made by means of those *post-mortem* examinations were so clear, and so undeniable, yet all the doctors have not, even to this very hour, absolutely given in, and perceived and acknowledged the truth of the matter; for there are still great multitudes of them, in every quarter of Christendom, who persist to consider the disease as a fever, and to treat it so in their therapeutical ministrations.

207. You know, however, that in all my public lectures on this subject, at the College, I have ever striven to persuade you that this is a grave error, which it is urgent to dissipate, if possible, because of its clinical mischiefs. But what hope is there that a public opinion, which has ruled and reigned over the Medical mind for centuries, should be abated or changed by an argument, or any number and power of them? In my opinion, one chief cause why our brethren do persist to regard this dreadful malady as a fever, consists in its name; and I, myself, though most anxious to see the error subverted and completely overthrown and destroyed, am, perhaps, aiding in these letters to uphold it, in that I have entitled this book *Letters on Childbed Fever*.

208. Mankind make use of language in order to communicate their ideas, wishes, or commands. Without language the race would ever remain ignorant, powerless, and barbarous. It is to

language that they owe civilization and all its attendant blessings. Without language men could not combine in society, nor effect, through the power of co-operation, any of those conquests over both brute and animated nature, which, whenever they are effected, do result in the establishment of both the public and the private weal. Words, then, are the signs of men's ideas; and if when we utter a word it excites in another's mind ideas corresponding to those we would by those words express and impart, then our words are the representations of our thoughts. But if words are used that excite ideas different from those material ones that exist in the mind of the speaker, then the words are traitors and deceivers; and they who hear them are misled, while those who use them are equally betrayed by them.

209. The word *Fever*, excites a certain material idea of *Zymosis* in the mind of the hearer of it. Typhus and typhoid fever, yellow fever, intermittent fever, bilious remittent fever—all these words excite ideas that are similar, in all who hear them, in that they all comprise an idea of *Zymotic* affections that are always signified by the word *Fever*. So also the word *childbed fever*, excites a material idea in the mind. But the impression varies according to the opinion taken by the hearer, of the *Zymotic* or other nature of the disorder intended to be expressed by the compound word. If one should say the word *childbed fever*, it would be difficult for the hearer to disembarass himself of the notion of a relation of some *Zymotic* cause to the affection, as leading to the whole train of pathologic consequences.

210. At present the question is, Is there such a thing as a *childbed fever*? To this question I am compelled to answer in the negative; wherefore, I must consider the word a false and misleading one, since it implies that the disorder is a fever, when, in fact, it is not a fever, but a *phlegmasia*, or pure inflammation. Hence, I say, the word *childbed fever* is a false and deceiving word, which ought to be banished from our medical vocabulary. But how can this be done? Who hath authority to do this great thing! Assuredly not I; and I know full well, there is not a man in the whole world, of influence and power so great as to change the habits and convictions of society, on any one subject, even the uses of a word; and of all things, none

is more difficult to change than language, which goes on to be repeated from age to age, by habit, even against conviction.

211. The English language already possesses the word childbed fever, and that is the reason why I use it; but I protest against its falseness; and I now put you, who are my pupils, on your guard against its deceptions, for there is no such thing as a childbed fever.

212. The disease called childbed fever is:—

1. Inflammation of the Womb—Metritis.
2. “ of the Uterine veins—Metro-phlebitis.
3. “ of the Peritoncum—Peritonitis.
4. “ of the Ovaria—Ovaritis.
5. It is all the foregoing disorders united in one case, or any two or more of them combined together.

In a woman affected with childbed fever, the disorder may consist in Endo-metritis; that is to say, in inflammation of the inner wall of the womb, or its inner layer, if one might so consider it; or in an Exo-metritis, wherein the outer stratum is affected; or, the whole mass of the uterus may be the seat of an inflammation that shall soften it in such a way as to allow you, in examining the organ after death, to push your finger through the texture of it in any direction, the substance of the tissues being, by the power of the inflammation, reduced to a state of pulp-like ramollescence that, so to speak, rots it, or destroys the cohesion of its tissues.

213. In some of the instances of childbed fever, the womb itself seems wholly to escape, while its larger veins are observed to be the sole seats of the inflammatory action, whose *post-mortem* results are detected as purulent deposit within the tubes of those veins, whose canals are thus converted into abscesses filled with pus: an abscess is a circumscribed cavity containing pus, and these vein-inflammations are among the most fatal of the childbed fever forms, principally because of the insidious nature of the attacks. In these examples of metro-phlebitis, the inflammation often proceeds very far, even to an incurable stage, before the woman feels any pain or becomes the subject of the least constitutional disturbance; so that, when the constitutional irritation has begun to manifest itself, or the pain to be felt, it is already

too late to interpose with any good effect, and the patient must be lost. But, of this hereafter.

214. Childbed fever often occurs, and passes through all its stages as a simple peritonitis; that is to say, the inflammation is limited to the serous lining of the pelvis and abdomen, the womb and ovaria wholly escaping from any serious lesion, so that, when the remains of the patient are examined after death, no traces of disease are to be found, except those referable to the peritoneum itself; but this is not always the case.

215. Many of the very rapid and uncontrollable instances exhibit, after death, the evidences of an antecedent inflammation of the womb, the uterine veins, and the peritoneum also; so that, as I before said, childbed fever may be metritis, or metro-phlebitis, or metro-peritonitis, or a peritonitis pure and simple, or all of these inflammations combined together in one and the same individual.

216. Whenever these inflammations occur in pregnant or lying-in women, then the disease is childbed fever, and it is nothing else; nor are childbed fevers ever anything else, except when they occur as accidents in typhus, smallpox, plague, and other typhous diseases, of which they are in some instances mere complications.

217. Hence, I say, a woman may be attacked with smallpox, and during the existence of that disease may or may not be seized with one or all of the above inflammations. In case of her being attacked, and dying so, she would die of a complication of smallpox and childbed fever.

218. The same is true of yellow fever, of bilious, typhoid, or jail or ship fever. Whatsoever disturbs the nervous and circulatory systems in a woman during the lying-in state, may or may not develop the abdominal inflammations that constitute our disorder.

219. Childbed fever, being inflammation of parts within the abdomen or pelvis, that inflammation must begin at or within some certain area. It is not known how great this incipient area is, or whether, indeed, there may not be several such areas serving as starting-points or radiant points, whence the phlegmasia spreads in various directions. Yet, it is most proper for you to consider

it as a highly probable thing, that the area of phlogosis always does begin somewhere below the plane of the superior strait, or, at least, upon some superficies of the *genitalia interna*.

220. I should be sorry that you, who are Students of my Class, and whom I hold it a sacred duty to instruct according to my conscientious conviction of what is both true and reasonable, as well as practically useful, should here take up the idea that I am speaking without due consideration, when I say that all the affections and accidents that can develop acute inflammation of the reproductive organs in lying-in women, do, at the same time, and by that very fact, tend to develop in them a childbed fever, which will be simple or complicated, as the case may be. Many writers judge that we ought to exclude some of the inflammations arising from accidents, as wounds, &c. &c. M. Gase is one of these, but I do not coincide with his opinion.

221. Instead of rashly, and without due consideration, making these asseverations, I have given much time to the examination of them in many of our best authorities, and among them, Dr. Robert Ferguson's *Essays on the most important Diseases of Women, Part I., Puerperal Fever*. London, 1839. I may take advantage of this epistolary method to say that this is perhaps the "crack" English book on the subject, and that our English brethren take it very much as a sort of medical Gospel in the disorder, praising it in their Reviews, and referring on all occasions to its authoritative opinions.

222. I sincerely hope and confidently trust I have in my bosom no cis-Atlantic prejudices against English writers on Medicine, which is a Republic, wherein all the people are equal. Yet I must say I consider it the most misleading and weakest book on childbed fever that has appeared since 1795. Its whole basis lies upon an hypothetical dogma, closely allied to the milk-dogma, that childbed fever depends upon a vitiated state of the blood; which is the reason why I look upon it as being tainted with the ghostly odor of *lait répandu, errement de lait*, or milk metastasis, of the old French milk-dogma. The three following propositions from his p. 83, embody Dr. Ferguson's views of the source and nature of puerperal fever:—

"I. The phenomena of puerperal fever originate in a vitiation of the fluids.

"II. The causes which are capable of vitiating the fluids are particularly rife after childbirth.

"III. The various forms of puerperal fever depend on this one cause, and may readily be deduced from it." At p. 81, he goes on to say:—

"The cause, therefore, of puerperal fever is, I think, simply a vitiation of the blood, which cause is demonstrably sufficient to produce all the phenomena; that phlebitis, or peritonitis, or metro-peritonitis, are only secondary effects of this one cause."

223. This is Dr. Ferguson's doctrine, in his own words, in a nutshell; and we will now let it stand so until a better occasion to discuss it shall arise, for Dr. Ferguson is not the only writer to attribute our malady to primary alterations of the fluids.

224. Dr. Fleetwood Churchill, in his proem to the Sydenham Society's vol., p. 34, says: "I must honestly avow that, while I admit the existence of local disease, I do think that epidemic puerperal fever is something more than that, although I may not be able to define exactly what it is." What do you think is this "something more?"

225. Dr. Ch. White, of Manchester, Syd. vol., p. 203, says: "It does not appear that this disorder can be ascribed to simple inflammation. The patients complain chiefly of a tension, soreness, and tenderness of the belly, and are seldom affected with those excruciating pains, which generally attend common inflammation of the bowels; but it evidently manifests itself to be of the putrid kind occasioned by human effluvia, by accumulations of acrid putrid bile, and of a putrid coluvies through the whole intestinal canal, and organs of generation, and is a malignant fever of the same genus as the jail or hospital fever." But why multiply citations? These are enough to show that the good authors disagree with the proposition I have laid before you, as to childbed fever, viz: that it is a phlegmasia and not a fever: still, the admirable paper of Mr. Dance, on Phlebitis, which you can see in the eighteenth and nineteenth volumes of the *Archives Générales de Médecine*, having thrown more light on the phlebotic form of childbed fevers than anything before or since his time, I intend to cite for you so freely from him as to show you very fully what were the views he took of the cases.

The valuable papers of Mr. Dance, are in the *Archives Générales*, vols. xviii. and xix. for 1828 and 1829, and bear the following title: "*De la phlébite utérine et de la phlébite en général, considérées principalement sous le rapport de leurs causes et de leurs complications.*" Par M. Dance, agrégé à la Faculté de Médecine de Paris." The results obtained and clearly explained by M. Dance, are so instructive, as well as so important in their applications to clinical practice, that I should be most happy to transfer every one of his remarks to these pages, were it consistent with the design of these Letters to do so; but, under the circumstances, I am compelled to restrain this desire, and only to say that M. Dance considered uterine phlebitis to be one of the most dangerous, and at the same time, least understood of the diseases of lying-in women. Nevertheless, it had, previously to him, engaged the attention of several intelligent physicians, who appear to have recognized it, at about the same time, in France and in England. Among the former, he particularizes M. Chaussier, Schwilgné, Ribes, and Husson; and Clarke and Wilson among the latter. Following soon upon some preliminary remarks, M. D. relates, at p. 476, vol. xviii., the history of a "Case, in which inflammation of the veins of the womb had extended to the ovaric veins, and their ramifications within the ovaries and broad ligaments, attended with intense cerebral symptoms. The woman, 22 years of age, and of a good constitution, entered the Hôtel Dieu, May 30, 1826, so delirious as to be unable to give any account of herself. It was found that she had left the Maternité two days after a natural labor, that occurred on the 18th of the month, and went to her usual occupations. She was attacked with violent fever, and afterwards with delirium; and on her being brought into hospital, was found greatly disordered; vociferations, cries, incoherent talking, obscenity, wild looks, brilliant conjunctivæ, extreme variable-ness of the features, wild laughter; then taciturnity and apparent calm; succeeded, in a moment, by renewed cries that filled the entire ward. The breasts were flaccid; the lochia consisted in a slight whitish and offensive oozing. The womb was felt above the plane, where strong pressure gave no pain. Pulse quick and frequent; skin moist; tongue natural. Such was the state of the patient, who, without any considerable change in these appearances, sank and died on the nineteenth day after her delivery.

At the *post-mortem* examination, marks were observed, of preceding irritation of the brain, with vascular engorgement; mucous membrane of the stomach of a blackish color, thickened, and a little softened; bowels natural; womb from five to six inches in length, and with walls from six to eight lines thick. The cavity of the womb was furrowed or puckered with the inequalities of a grayish false membrane, that lined its surface. Beneath this membrane, the substance was blackish, friable, and softened to the depth of several lines. The surfaces exposed by incisions, exhibited many gaping orifices of vessels, from which escaped a purulent matter, upon pressure. The cavity of these vessels, any of them, was lined with a thick layer of pus, almost as consistent as false membrane. The substance surrounding the cut-off vessel, appeared as a yellowish-brown aureole: wherever the substance of the womb was incised, vessels in this condition were discovered. The ovaries were two-thirds larger than natural, and their tissue riddled with little abscesses; but pus came, also, from their veins, when incised. The Fallopian tubes and their ligaments were sown with numerous small abscesses, chains of which were seen. The Fimbriæ were red, and composed, each, a thick mass, infiltrated with pus. The two ovaric veins were transformed for full half their length, into long and solid cords, within which thick false membranes were observed adhering to their inner walls. These appearances were not discovered in any of the other veins of the subject. At p. 480, M. Dance describes the orifices seen at the utero-placental disk, on the inner surface of a new-delivered womb, and from which the fluid of an injection, made downwards from the vena cava, readily escapes into the womb's hollow. He gives an account of the appearance of them presented after the detachment of the placenta, some of the vessels being so open as to admit the point of the little finger; and these openings lead into the veins and sinuses of the uterus. These vessels do not convey their outer coats within the substance or texture of the womb, but only their inner or lining membrane (*membrana vasorum communis*—Endangium), or true bloodvessel. This womb-vessel rests upon the womb-tissue, which serves as its basement tissue; thus taking the place of the other coats, or ordinary basement, which, as I remarked, is left outside. These sinuses and large veins, therefore, remain very

large after parturition, as they cannot contract, except as the involution of the womb, which constantly reduces their caliber, enables them to grow gradually smaller, or more delicate. The relation of Dance's 1st *Case*, appears to me quite suited to give you very plain views of the disease, its seats, and tendencies. The delirium, one of the most obvious of its symptoms, is highly characteristic of phlebitis, with pyæmia.

Case 2, is at page 485. The patient died a few hours after her delivery; had had fever, pain, and delirium. The alimentary organs were found to be natural; but the uterus was greatly disordered, and so soft as to break by bending, or by being folded. The finger could be pushed through its tissue as readily as into a hepatized lung, so great was the ramollescence. The right ovarian vein was inflamed, and contained pus, with coagula and false membrane. The vena cava was also inflamed, and had pseudo-membranous deposits on its walls, but no coagula, or fluid pus.

Case 3 was that of a patient attacked, four days subsequent to delivery, with rigor, headache, palpitations, fever, great depression, hot and dry skin, and pale but moist tongue. The belly was soft, and not painful. The hypogastrium, raised by the underlying uterus, was salient above the pubis; epigastric distress; small and very offensive lochia. The pulse was frequent. Upon the twelfth day after delivery, she was delirious, and had subsultus of the mouth and tongue. Pressure on the hypogastrium gave no pain. She was sometimes delirious, and at others, had wanderings. At night, a chill; and she died in fifteen days after her delivery. There was no sign of peritonitis. Outer aspect of the womb, which was large, quite natural; but the inner wall exhibited a violaceous hue, and was full of holes, the size of a writing-quill, containing a whitish viscid pus.

The 4th Case was one of inflamed womb-veins, and ovarian vein; the latter, throughout its entire length. There was pus in the articulations of the wrists, and in the second metacarpal series. There was pus in the symphysis of the pubis; but there was no peritonitis.

The 5th Case, by M. Dance, was one in which the inflammation was limited to the region of the womb covered by the placenta. It was attended with the usual sign—delirium. In Case 6,

inflammation of the uterine veins passed onwards into the ovaric and hypogastric veins. There was brown ramollescence of the uterus; numerous pus-cavities, of a peculiar appearance, were seen in the lungs; a double pleurisy; pus in the symphysis pubis; delirium; and death on the eighteenth day from delivery. M. Dance supposes the pneumonia to have been derived from the pus, transported along with the blood, into the lungs.

The 7th Case was inflammation of veins, lying within the substance of the uterine walls, the round ligament and right ovary, and the right ovaric and hypogastric veins. There was brownish ramollescence of the womb, and traces of peritonitis; pleurisy, and numerous small purulent foci in the lungs. She died twenty-five days after her delivery. Case 7. Uterine phlebitis, chiefly of vessels in the left half of the womb, extending into the ovaric vein; slight traces of intro-pelvic peritonitis; double pleurisy, with pus in small cavities in the lungs. There had been some hypogastric pain, upon pressure. Delirium. Died twenty days *post partum*. Case 9. Subacute metritis, with uterine phlebitis. Died twenty-ninth day after childbirth; had had very little pain; great weakness; dry, hard tongue; intense thirst, small lochia, and a pulse at 120. The skin was yellow; lethargy; involuntary stools; speech not to be understood. Complaining of no pain; she sank on the twenty-ninth day after the birth of the child. There were two small abscesses in the lung, and some traces of peritonitis. A large purulent collection was found in the veins of the womb, which veins proceeded towards a double tumor upon the outer surface of the organ, that was composed of an assemblage of vessels, all filled with pus, and which, after uniting into one single vein proceeded to open, at the distance of nine inches, into the vena cava. This vessel was the right ovaric vein, filled throughout its entire length with pus." In Case 10, there was blackish ramollescence of the womb; inflammation of its veins, of both the ovarics, and of the right hypogastric; abscess in the cava; gangrenous softening of the spleen; pericarditis; pus in the left pleura; numerous abscesses in the lungs, chiefly the left. Ramollescence of the *Trigone cérébral*. New abscesses under the skin of both forearms, and also between the intercostal muscles and the pleura. These lesions resulted in death, about four months subsequent to the parturition. In Case

11, softening of the womb; peritonitis; pus in veins in the pelvis; abscesses in right lung, liver, and brain. The labor had been a very laborious one. At first, the woman complained of dull pain in the hypogastrium, and had a continued fever, attended with profuse sweats at night. The emaciation was rapid; then came on a gradual prostration, with altered physiognomical expression, a low delirium, abundant diarrhoea, and death soon after her delivery. Cases 12 to 21, all exhibited the familiar signs of this cruel disorder, all of them being accompanied with the usual symptoms during life, particularly the delirium, and exhibiting the *post-mortem* evidences, except Case 21, which was cured at the close of about two months. This woman was much bled, during her illness, both with venesection and leeches. M. Dance's third and last article, vol. xix. 1829, contains his excellent reflections upon the foregoing histories. As to the treatment, he thinks that venesection is much to be preferred to leeching, because it disengorges the womb much the most rapidly, and so, favors its involution. By means of venesection the caliber of the veins is lessened; and hence, the propagation of the inflammation along the tract is prevented, and the transport of the pus less to be dreaded. He also approves of baths, fomentations, emollient cataplasms, cooling drinks, and scrupulous attention to the diet and regimen of the woman. I trust, my dear young gentlemen, that the perusal of even this rapid and very meagre sketch of M. Dance's opinions and experiences may convince you of this one thing at least, viz: that you will, in practice, meet with cases of uterine phlebitis, and that you must cure them or lose your patients, and that you will not cure any of them unless you first learn that all these phlebotic affections are not fit to be called membranous inflammations, but only and truly phlegmonous inflammations; and as they are often connected with peritonitis, a purely membranous or superficial phlegmasia, you will be able to cure the latter, and yet leave the former to destroy your friends, if you do not act wisely and well.

I have not been able to obtain the original authority for a strange passage in Dr. Gardien's *Traité Complet d'Accouchemens et Maladies des Filles, des Femmes, et des Enfants*, who, in vol. iii. p. 382, has the following words: "Walter, who opened more than five thousand five hundred (*cinq mille cinq cents*) bodies of persons who died

with peritonitis, always found the greatest resemblance between the fluid effused into the abdomen, and that noticed in women who have perished with puerperal fever." I said this is a strange passage, since it is difficult to imagine how one individual could possibly have opportunity to make such an immense number of dissections in a single disease. If there is no mistake in the statement, it may serve to show how slow is the progress of knowledge, for we are not yet agreed after the *cinq mille cinq cents*.

226. Another great flood of light was likewise thrown on the subject of childbed fevers twenty-five years ago, by M. Tonnellé. Indeed, one might safely conclude, that from the era of Hippocrates, down to the date of this publication, no such considerable stride in advance, as these, was made by any physicians of any age or country. I mean to speak of a step forwards in the knowledge of the real seat and nature of the disease. As to progress in practice, I always believe we are indebted more to Alexander Gordon, than to any other person whatever. In every point of view, however, a famous and certainly a most fair and conscientious book is that of M. Tonnellé, who served at the Maternité, at Paris, with M. Desormeaux, in a horrible epidemic of childbed fever there, and who acknowledges that eminent person to have been his guide, his teacher, and friend in the affair. Dr. Desormcaux was, at the time, at the head of the establishment la Maternité. Tonnellé's paper bears the stamp of truth and accuracy, and I advise every one of you to read it as soon as possible, in the twenty-second and twenty-third volumes of *Archives Générales*. The epidemic raged in the Maternité Lying-in Hospital in 1829, where Tonnellé was so diligent in learning, that he dissected the bodies of 222 women, who perished there during the prevalence of the malady. These dissections were most carefully made, and by the most able hands. The examinations disclosed the fact that—

In 193 cases there was peritonitis.

In 197 " there was alteration of the womb and its appendages.

In 79 " there was simple metritis.

In 29 " superficial softening.

In 20 " deep-seated softening.

In 58 " inflammation of the ovaria.

- In 90 cases pus in the veins.
- In 32 " pus in the lymphatic vessels, and at the same time,
- In 3 " pus in the thoracic duct.
- In 9 " inflammation and suppuration of glands, as the lumbar, inguinal, &c. &c.
- In 32 " where the veins were inflamed, the womb was also inflamed.
- In 11 " where the veins were inflamed, the womb was in a state of ramollescence or putrescency.
- In 5 " where the veins were inflamed, there was also metritis and ramollescence of the womb.
- In 34 " where the veins were inflamed, there was peritonitis only.
- In 8 " the veins alone were inflamed.
- In 20 " the lymphatics were in suppuration concurrently with suppuration of veins.
- In 13 " lymphatics in suppuration, together with suppuration of the womb.
- In 6 " lymphatics in suppuration; there was also ramollescence of the womb without suppuration.
- In 3 " lymphatics in suppuration with simple peritonitis.
- In 2 " lymphatics in suppuration without other complication.
- In 29 " Ovaritis with simple peritonitis.
- In 27 " Ovaritis with divers alterations in the womb.
- In 8 " Ovaritis with simple metritis.
- In 7 " Ovaritis with ramollescence.
- In 12 " Ovaritis with suppuration of vessels.
- In 16 " Ovaritis with all the preceding forms combined.

227. M. Tonnellé remarks, that the above results show, that affections of the womb, in childbed fever, exceed in number the cases of peritonitis; that they are most generally combined in the cases, and that each of them may be wanting in childbed fevers. He also considers it to show, what is very remarkable, that in 134 of the cases, the veins or lymphatics contained pus.

228. I should never finish were I to go on with quotations to prove that individuals who die with childbed fever, show the manifest and undeniable marks of inflammation within the belly;

and I shall not take the trouble to endeavor to convince you that these phlegmasias do take their rise in parts contained within the pelvis, or that the primary areas of the inflammation are *intro-pelvic areas*, which spread, on the one hand, far and wide within the abdomen when they are peritonitic, or, on the other, effect their deadly purpose without rising above the plane of the superior strait, in all those cases that consist in pure metritis or metro phlebitis or ovaritis, under the various forms of putrescency of the womb, phlebitis uterina, &c. &c.

229. M. Tonnellé's reports of his *post-mortem* examinations appear to me to be perfectly clear and truthful. I say so, not only because his representations agree with those of other writers, but because they tally with the repeated observations I have myself made during a great number of years that I have practised Midwifery. I have met with multiplied instances of putrescency; of softening, whether of the whole texture, or the inner or outer stratum only, of the uterus; of pus in the veins, and pus in the lymphatics; destructive and softening inflammation of the ovaries; broad ligaments full of pus, and inflamed peritoneum, with and without affection of the womb or its bloodvessels.

230. I shall here relate, for you, a case of my own, with an account of the dissection. The woman perished with precisely the same constitutional disorders as I have so many times observed in cases of sporadic, and likewise epidemic, and even hospital childbed fever.

Mrs. G. S. B., æt. 28, had, at various times, in the latter weeks of her second pregnancy, complained of severe pain in the left iliac region, in textures somewhere near the upper strait of the pelvis. In complaining of the attacks of pain, she also frequently had fainting fits; but recovered from them without very alarming accidents. On Tuesday (8th of June, 1841) she gave birth to a healthy boy, after a short but natural labor. Nothing unusual attracted my attention in the course of the labor, and I had no suspicion that those pains and the faintings were connected with some very serious mischief, particularly as they gave out no signs during the efforts of parturition. Thursday, 10th, abundant secretion of milk, with a slight milk fever. Monday, June 14, a flatulent belly, and considerable fever, but not so much as to

excite in me any alarm: examined the hypogastrium, and also the vagina, and found pain when I pressed upwards the left *cul-de-sac* of vagina, or thrust moderately against the left side of the pelvis, towards the ischial plane.

231. Tuesday, 15th of June. This morning, no dysury, no pain; pulse nearly natural: the same at 4 P. M., at which time she was in excellent spirits, the belly being soft, and not painful to the touch; pulse moderate. She said she was so much better, that I need not visit her again until to-morrow: the bowels free. In the afternoon, she ate a saucer-full of strawberries; was comfortable, and slept soundly until 4 A. M., Wednesday, 16th of June, when she awoke, saying she was perfectly comfortable, and "never felt happier in her life." She asked for the child, nursed it, and then said: "Nurse, take the boy; and I shall turn over, and have a delightful morning nap." At the moment the nurse took the child from her, she was seized with the same excruciating pain in the left iliac region, as before her confinement. I arrived soon after 6 A. M., and found her bathed in colliquative sweat; pulse feeble, and exhausted; pains intolerable; belly greatly distended. Pulse, which was quick, grew rapidly weaker and weaker, and the body and limbs cold. An enormous ephidrosis now came on, and lasted until 1 A. M., of Thursday, June 17, when she expired.

232. I repeat that this woman died with all the constitutional, as well as local symptoms of a true childbed fever. She survived the attack only twenty-one hours—say from 4 A. M., Wednesday, 16th, to 1 A. M., Thursday, 17th of June.

233. Prof. Pancoast made the examination; both the ovaries enlarged and red; peritoneum slightly glued by means of exudative matter. The involution of the womb had gone on naturally. The left Fallopian tube was as big as my thumb, and the cavity of it would admit a finger, instead of a bristle or small probe, as usual. It was full of pus, and the outer extremity was open, so as to discharge the pus into the belly. The tube had probably already collapsed, and was of a liver hue. This enlarged left Fallopian tube, and the corresponding broad ligament, were the seats of the most intense inflammation.

234. This Fallopian tube had, doubtless, been long inflamed,

and that had given rise to the pains she had complained of some weeks before her delivery; it was converted into an abscess, the canal of the tube serving as the walls of the abscess. It had opened, and discharged its purulent contents, as she turned at 4, A. M., in giving the child to the nurse. A phlegmasia was the result, which killed my patient in twenty-one hours with a childbed fever, or puerperal peritonitis. This is a pure sample of what I called, at sec. 200, accidental, or sporadic childbed fever. I have seen, in forty-odd years, many cases and dissections of childbed fever, and I never saw a more exquisitely marked case than this. Let no man, who denies my position that childbed fever is phlegmasia, say that Mrs. G. S. B.'s disease could not be like the bad cases that occur in hospitals. I aver, that the worst hospital cases cannot present symptoms more strikingly characteristic than those I saw here; I mean the altered physiognomical expression, the utter exhaustion, the eructations, passing into black vomit; the pallor, and the failing pulses, and the clammy sweats, and rapid extinction, which, by many, would have been regarded as evidences, or, rather, proofs of putridity, or vitiated blood.

235. I have lying before me "*A Treatise on the Epidemic Puerperal Fever of Aberdeen*. By Alex. Gordon, M. D., Physician to the Dispensary, London, 8vo. 1795." Such is the title to Gordon's famous work, that I have so often urged you to study, as a sort of Gospel book for this particular line of clinical information. You have many times heard me say that, to Gordon we are really indebted for the greatest and most useful reforms in our practice in the disease; and I shall have much still, to say about his great services to us, in a future page. For the present, I have brought him before you that you may see what his opinions are concerning Isabel Allan's case, a woman, æt. 38, who was confined on the 22d Sept. 1790. Twenty-four hours after delivery, she had a violent rigor, succeeded by acute pain in the hypogaster, especially on the right, with great fever. She had been sick 30 hours when Gordon was called, on the 24th. He found the belly considerably tumefied; pulse 140, and hard; sick, and vomited green bile; loehia suppressed, and urine high colored. She had all the symptoms of puerperal fever. He ordered her to be freely bled: abdominal fomentations, and a diaphoretic draught at night. In the morning, 25th, happy to

find her apparently much better; Pulse 124, pain of belly much abated; sweating profusely. This he endeavored to promote by emetic tartar, in small doses. In the evening, there had been a rigor of long duration, followed by considerable increase of fever and pungent pain, with tension of the belly. A large blister to the abdomen. Next morning, 29th, all the symptoms worse; swelling increased, and pulse 160. In 24 hours she died.

"I found the peritoneum and its productions, the omentum, mesentery, and meso-colou in a state of inflammation. The omentum had lost about half its substance by suppuration; the mesentery and mesocolon, and that part of the intestinal canal with which they were connected, were very much inflamed. But the disease appeared more especially to occupy the right side; the right ovary had come to a suppuration; the colon, from its caput, along the course of its ascending arch, was much inflamed, and beginning to run into gangrene. A large quantity of pus and extravasated serum appeared in the cavity of the abdomen, which, when taken out and measured, amounted to two English pints. The peritoneal coat of the uterus was inflamed, and the organ itself not so compact and contracted as it ought to have been. Upon opening it, its cavity was found covered with a black colored substance, which, at first sight, had the appearance of mortification, but when wiped off was found to be nothing but the membrana decidua, in the state in which it naturally is about this time." p. 28.

236. Here you see the undeniable proofs of an inflammation so great in violence, and of so vast a superficial extent, that you cannot but very readily attribute to it all the signs of constitutional disturbance that Gordon describes; disturbance that must certainly lead to the dissolution of the patient, if unchecked and uncured by remedies. It is true you can find in many more recent authors, accounts of dissections more elaborately told than this one, yet this is a very clear one, and, it seems to me, a very convincing one. You are to remember that this was one of the epidemic forms, and you should know that Gordon lost 27 out of 77 women that were sick with the disease.

237. What was the cause of the chill she had on the 23d? and why had she that pain in the lower belly? Was not the chill

the evidence of that *concurritur* of the powers that we notice in a forming inflammation? Are you surprised that there should be so much pain—so rapid a pulse (140)—so sick a stomach—such tension of the belly, sweats—and at last, exhaustion of the nervous forces, and then death to end all? Every case you are doomed to meet with hereafter, will be, in many respects, just like this, though modified by the particular seat, as metritic, phlebitic, ovaritic, peritonitic, &c.

¶ 238. Some years ago, Messrs. Barrington & Haswell published a volume here, containing the works of Gordon, Hey, Armstrong, and Lee, on puerperal fever, and I made for it an introduction, which contained some of my views on it at the time, 1842, which is now 12 years ago. At p. 25, there is an account of a case from my note-book, and as that case made, at the time of its occurrence, a strong impression upon my mind, and as I have a still lively impression of the person and the incidents, I am about to transcribe it here; for I think it will do you good to read so true an account of a case. It will do you good, I say; for it will show you how this disease acts when it has taken strong hold of a strong woman.

239. Mrs. W. G. aet. about 20 years, healthy and strong, was delivered, Thursday, Nov. 4th, 1830, of a girl; in a labor of four hours. Friday, 5th, and Saturday, 6th, quite comfortable; breasts contained a little milk, but they were not swollen or painful. On Saturday a dose of castor-oil was taken, which operated ten or twelve times that day. Diet, tea and bread, and oatmeal gruel. No fever. Sunday, 7th, 10 A. M. Had a chill in the night; had rested badly, and now has pain and soreness in the right iliac region: the part very tender, resonant, and distended. The fundus uteri sore to the touch. Lochia bright and free; urine plentiful; tongue whitish, moist, soft, and supple; headache, thirst; dorsal decubitus; motion of thighs gives pain in the belly; to turn or to rise also gives pain; pulse 148, with a vigorous stroke. I bled 18 ounces from a large orifice, whereon she became faint, and I bound the arm. In a few minutes after the bleeding, pulse 112, but it soon rose again, and beat 152. To relieve the tympanitis, an enema, which produced two dejections with relief. A flannel bag filled with wheat bran, and soaked in boiling vinegar

and water, being first well pressed, was laid on the abdomen, and changed occasionally for a fresh one. At 20 minutes past 3 P. M., pulse 145, with a smart stroke; tenderness of the belly neither less nor greater. I now took 12 ounces of blood from the arm, *pleno rivô*; but was obliged to stop for the faintness, though I had drawn the pillows from under her head, in hopes of getting a better bleeding. In 15 minutes after the v. s. pulse 144. At 6 P. M., ordered 8 grs. calomel with opium jss. grs. for a dose. To drink gum water. At 6½ P. M., has slept, and feels decidedly less pain and soreness; but as the pulse was full and strong, I drew away 22 fluidounces of blood, which was carefully measured. It made a firm clot, and had a thick coat of size. She now took of manna ʒss, sem. anisi ʒj, magnesia carb. ʒss, aq. bullient. ʒvj. An infusion being made of the manna and anise, the magnesia was added to the strained liquor. The dose was one fluidounce of the mixture every hour, until the bowels should be moved. At 10½ P. M., pulse 136, full and strong; the right mamma filling and hardening; the left, soft and flaccid, yet the gland developing itself favorably; no headache; thirsty; soreness and pain, carefully examined, much lessened; lochia free; decubitus still dorsal. Monday 8th, at 8 A. M., pulse 130, and softer; has slept a good deal; no pain except from firm pressure on belly; thirst lessened; both mammæ full and hard. 3 P. M., pulse 120, full and strong; no pain, not even from pressure; tongue clean; several dejections; not thirsty. Tuesday, 8th, 9 A. M., pulse 124 and strong; tongue a little furred; plenty of milk; breasts soft; no pain; several dejections. 9 P. M., pulse 111; nipples sore. Wednesday 9th, 10 A. M., sitting up; no pain; pulse 126. In a few days after this memorandum she was quite well. Between 11 and 6 o'clock on the first day of the disease, I drew away 52 ounces of blood from this woman's arm, without which I think she must have died.

240. If you will examine this case, I believe you cannot doubt it was a case of childbed fever, nor that it consisted in intense inflammation that had laid its incipient areas upon the intro-pelvic peritoneum, and was rapidly spreading out those areas above the brim of the pelvis. My venesections and other remedies checked its progress, and, at last, impressed upon it a disposition to recede by resolution; so that, by 3 P. M. on Monday, the resolution of the inflammation was completed, and the consti-

tutional irritations had fallen with it. If I had not bled the woman so well, would she not, by 3 P. M. of Monday, have been affected with all those metro-abdominal inflammations, that Gordon found in his dissection of Isabel Allan's body? I have no doubt it would have been so; nor have I any doubt that my art rescued the woman from imminent destruction. I have rescued many such, and so will you, if you but make yourself acquainted with the truth of the matter. What truth? Why this truth—childbed fever is a phlegmasia, and is not a childbed fever. If you can cure the inflammation, all the rest will be cured, "*ipso facto*."

241. I do not pretend to know what made this woman sick; all I do know about it is, that she was a very rosy, beautiful, and most healthy creature, when in labor, and for some time after the child was born. Her mother, who nursed her, gave her too large a dose of castor-oil. I presume, that if she had not taken the dose she would not have been ill, and that the disturbance both nervous and sanguine, produced by the violent purgings, laid the areas of the phlogosis, somewhere within the pelvis, and so she got an accidental, generally called, sporadic childbed fever, or puerperal peritonitis. No vitiated state of the blood, as Ferguson supposes, *vide* sec. 222, had anything to do with the matter; nor any miasm, contagion, halitus, nebula, or halo. It was a pure accident that gave rise to an inflammation; but, if I had not cured her, she would, by Monday afternoon, have been in a deeply typhous state, and then, indeed, her blood would have been vitiated and even ruined. Gooch tells us, p. 7, that, "Dr. Clark suspected that the disease (epidemic in 1787) might be occasioned by the purge that is always given to lying-in women on the second day after delivery, but glysters were not more successful." I have seen several persons attacked in consequence of the inopportune, or excessive use of purgatives.

242. Let me say further, that I cured this inflammation because I got to the house not later than 10 A. M., when the disorder was only a few hours old; and also, probably, because it had not yet become a phlegmon of the substance of the womb, or of any of its large venous sinuses. I mean by this to say, that, had some of the vcins begun to inflame, I should, in all probability

have failed, as I have on several occasions. One of those occasions made a profound impression upon me, and I will now tell you about it. Two years and a half ago, I attended, in her confinement, a tall, slender, weakly woman, in her third pregnancy. She had a good labor, but shortly afterwards was seized with childbed fever; that is to say, she had intense rigor, with most rapid pulse, hypogastric pain, dorsal decubitus, altered physiognomy, and, indeed, the alarming symptoms of a direful attack. The chill was not yet ended when I arrived at her house, and I immediately bled her, though she was yet shivering occasionally. But, as I could not, on account of her fainting, get more than 16 or 18 ounces, I felt heartsick for fear, as to her safety. I should have had no such fear, could I have abstracted 25 ounces, and I never have any when I can do so early; but, if I find myself compelled to desist far too soon, that is, before I can get some 24 or 26 ounces, I always consider myself a baffled man, and retire with inexpressible anxiety as to my patient's security. Not so, never so, when I can say as I bind up the vein, "I have succeeded in my venesection to my heart's content;" for then I always go away confident in the result for my patient. Nevertheless, the venesection, with other remedies, did afterwards appear to me to have cured her; for her pain, and soreness, and fever went away, and her milk and lochia came back again. Yet in a few days she became hysteroid (they always become hysteroid when pus is mixing with the blood); and so, in some days afterwards, she died, as I have seen many die, from pyæmic fever. Upon dissection, no traces of peritonitis were discoverable, but everything in the belly was fair and normal. Even the womb, whose involution had gone on well, seemed so natural that one would suppose it a healthy specimen. Even when incised, it looked well. The inner wall was as it should be. Many incisions disclosed nothing, to our great surprise, until, at last, the bistoury laid open a congeries of veins, on the right side of the womb and in the broad ligament, which were full of perfectly laudable pus, and then the truth was known. She had perished with pyæmia. I did treat and did cure a violent and extensive peritonitis, but there was also an incipient metro-phlebitis that I failed to cure. That phlebitis went to the pyogenic stage, and so, furnishing pus-corpuscles, which were washed away into the

torrent of the circulation, infected the whole mass of the blood, and destroyed my patient. I shall ever believe that if I could have taken 25 ounces at that first bleeding, I should have eradicated, not only the peritonitic, but also the phlebitic affection. It is true that a well-designed and well-executed venesection is often able *jugulare febrim*.

243. I find I have already written a very long letter, without exhausting the subject concerning which I desired to communicate my sentiments. I shall, for fear of fatiguing your attention, release it now, and proceed in my next letter with further observations on the question: What is childbed fever?

C. D. M.

LETTER VIII.

OPINIONS OF GOOCH FAULTY FROM HAVING PRECEDED DANCE
AND TONNELLE—GASC, RACIBORSKI, ON THE VEINS—UTERINE
PHLEBITIS—PUTRESCENTIA UTERI—SEROUS OR EXO-METRITIS—
MUCOUS OR ENDO-METRITIS—METRITIS PARENCHYMATOSA—
AFFECTION OF THE SUBJACENT ORGANS IN PERITONITIS.

244. DR. ROBERT GOOCH, of London, appears to have been a physician who attracted to himself everybody's love and confidence; and, I think, that the present century has not produced a medical man, in all England, who had so general an esteem as he. His writings are charming by the simplicity and beauty of their style, and by an air of truth and sincerity that is seen in every page. His *Account of the Diseases peculiar to Women* is an octavo-volume of 432 pages, printed at London, 1829.

245. I am going to cite for you one of Dr. Gooch's paragraphs; not because it represents what I consider to be a truth in regard to childbed fever, but because I have so great a respect for the memory of this good physician, that I wish you to know what his sentiment was, though my own differs widely from his. But here is the excerpt from his page 96:—

“The most remarkable circumstance that the experience of the last few years has taught us about peritoneal fevers is, that they may occur in their most malignant and fatal form, and yet leave few or no vestiges in the peritoneum after death. The state of this membrane, indicated by pain and tenderness of the abdomen, with rapid pulse, appears to be, not one uniform state, but one which varies so much in different cases, that a scale might be formed of its different varieties; this scale would begin with a little more than a nervous affection, often removable by soothing remedies, and when terminating fatally, leaving no morbid appear-

ances discoverable after death. Next above this, a state, in which this nervous affection is combined, with some degree of congestion; indicated, in the cases that recover, by the relief afforded by leeches, and in the cases which die, by slight redness in parts of the peritoneum, and a slight effusion of serum, sometimes colorless, sometimes stained with blood. Above this might be placed those cases in which there are, in the peritoneum, the effusions of inflammation, without its redness; namely, a pale peritoneum, and no adhesions; lymph, like a thin layer of soft custard, and a copious effusion of serum, rendered turbid by soft lymph. Lastly, the vestiges of acute inflammation of the peritoneum, namely, redness of this membrane, adhesion of its contiguous surfaces, a copious effusion of serum, and large masses of lymph."

246. There, gentlemen, is Dr. Gooch's opinion expressed in his own words, and if he is right in it, you are to come to the conclusion that a woman may have a childbed fever, according to the first degree of his proposed scale, which is only a nervous affection, removable by soothing remedies (I know not what he means by soothing remedies), or, if not so cured, ending in death without traces of lesions in the peritoneum. Will you agree with him to call this a puerperal fever?

247. Now, I wish you to observe, that Dr. Gooch's book was published in 1829, and that as M. Tonnellé's articles in the *Archives Générales*, did not appear until 1830, a year after Gooch's book was published, he could not have been so well informed on the subject of childbed fever, as he must have become after reading Tonnellé's disclosures.

248. I wholly deny that a woman can have a childbed fever, according to Dr. Gooch's first scale, and should deeply lament to find a Student of mine taking up such an idea; since I cannot conceive of his entertaining it, without at the same time admitting childbed fever to be a fever, which is a doctrine both false and dangerous; false as to the pathology, and dangerous as to the practice; that is, false both exegetically and practically.

249. There is a writer on puerperal fever, who is often referred to as authority; but, though in general a sensible author, his pathological views here are no better than Gooch's. However, in his "*Dissertation sur les maladies des femmes à la suite des Cou-*

ches, connues sous le nom de Fièvre puerpérale," J. Charles Gasc very sensibly says: "Observez que cette inflammation locale est un phénomène tellement constant dans la maladie qui nous occupe, que, si elle n'existoit point, on ne pourroit point dire qu'il y eut fièvre puerpérale." The inexcusable fault of Gasc's treatise is, that it formally pleads that childbed fever is inflammation of the peritoneum only, and that the womb and the bowels have no concern in it. He says, 279: "Nous avons démontré que cette maladie n'étoit qu'une phlegmasie du peritoine."

250. The cause of Dr. Gooch's mistake, and Gasc's also, was that they knew less of phlebitis, than has been known since their day; and I have only, perhaps, to turn your thoughts to the case which I related in sections 225 and 242, in order to let you at once know, that a woman may die of childbed fever, and leave no morbid signs, for those, at least, who do not search for them in the right place. In that case, I was near giving up the search, after the womb had been repeatedly incised, and laid open through and through, when, at length, a stroke of the bistoury showed me veins filled with pus. In fact, those veins had secreted it from their inflamed *membrana communis*. And here let me put you in mind that the great venous vessels, that ramify within the tissue of the uterus, consist of the endangium alone; for it is quite true, that a bloodvessel penetrating into the substance of an organ, or viscus, does leave all its adventitious coats outside; carrying in, only the true, real bloodvessel, which is the endangium. Now, some of the uterine vessels are so large, that, in a recent womb at term, and not contracted, one can, when it is cut open, readily thrust a finger into the cavities of the sinuses. This, I know to be true, having done it myself. In looking at these vessels, it is evident that they consist merely of the endangium, or *membrana communis*, which, requiring no other base to rest on, does use the substance of the womb as its basement tissue.

251. If you will study M. Raciborsky's paper on the veins, in the *Mém. de l'Acad. Roy. de Méd.* vol. ix., you will learn that he fully establishes this doctrine, in regard to the vessels; and I wish very much that you should satisfy yourselves with regard to it, for it throws much light on the subject of metro-phlebitis, by showing

that where the veins of the womb become affected with phlebitis in the deep-seated structure of the organ, the basement tissue or organ itself must also soon become affected, and that violent and universal phlebitic inflammation of the viscus may hasten the whole uterus into that state of ramollescence, that is called putrescency of the womb. My friend, Dr. Haines, some time since a practitioner here, brought me the uterus which he had taken from the body of a woman who died under his care forty-eight hours after her delivery. The whole inner stratum, if I may so call it, was soft or rotten, while the exterior, and much larger portion was perfectly sound and normal. All this change must have taken place subsequent to the labor, in a disease lasting only forty-eight hours.

252. Where the phlebitis is situated in veins lying on the outside of the womb, and within the folds of the broad ligaments, the destruction of life must go on more slowly, for, as in my case, sec. 242, it does not necessarily involve the womb itself, and kills at last, only by purulent infection of the blood. It often admits of recovery by the rising of the pus to the groin or iliac region, where it is discharged through the skin, escaping along the course of the abdominal canal, or underneath Poupart's ligament.

253. I cannot explain why it does sometimes happen that in childbed fever, the inner layer of the womb only suffers this ramollissement or putrescency, and at other times only the outer layer, and at others, again, the whole womb. These peculiarities cannot be independent of a state of the vascular apparatus of the organ. If the whole endangial tissues within a recently delivered uterus could be supposed to be inflamed, I can readily conclude that such inflammation might, in certain cases, reduce the entire organ to a state of putrescency, in the course of less than twenty-four hours, as in Kiwisch's case. Perhaps, indeed, this is the true rationale of uterine putrescency. See sec. 293.

254. No one ought to be surprised to find a woman die rapidly, with such a state of such an organ, which ought to suffice, one would think, to overthrow the functions of the entire nervous mass in the course of a few hours. The nervous shock in such a case, would be equal to the nervous shock experienced in certain railway accidents, or in the crushing of important joints, &c.

255. When a patient dies with childbed fever consisting of puerperal peritonitis alone, and there are many such victims, she dies because the disorder extends to and involves many important organs, and in the course of it, interferes with many essential functions, as I shall endeavor to explain; but when she dies with putrescency of the womb, she perishes speedily under shock, and perhaps, partly from pyæmia.

256. Whenever the serous lining of the belly inflames, it has a tendency to spread far and wide, wherever that membrane extends, and it would undeniably always do so, were its progress of extension not interrupted by a cure on the one hand, or on the other, by the death of the individual, who succumbs, earlier or later, according to her power of resisting the destructive impression of so vast an area of phlogosis: some women die before it has ascended far above the plane of the strait, while others resist the subverting and destroying force of the phlogosis, until it has pervaded every portion of the peritoneum. This is the reason why, in your dissections, you shall find either small patches of inflammation, here and there, or signs of its extension throughout the whole abdominal cavity.

257. You would call such a case by the name peritonitis, or puerperal peritonitis; and if inattentive to the meaning and import of the word, you would fall into the error of regarding it as a serous disorder only. But I beg to say this would be a grave error, and one that might blind you as to the real state of the woman; for all such cases are something more, and more serious than would be a serous inflammation, regarded *per se*, or singly. For example, the intestinal canal consists of the mucous, areolar, muscular, and serous tunics, all of which, when combined, make up the organs called the bowels. But, if a Student in the dissecting room should show you an intestine from which he had dissected off the whole of the serous, the muscular, and the areolar tunics, you would say to him: "It is not an intestine that you show me, but only the mucous tunic of one; you have not got the serous part of the bowel nor its muscles." Hence, it is quite true to say that the peritoneal coat of the bowel is part of the bowel; and further, when this peritoneum is inflamed, it is an inflammation of the bowels also, and constitutes what De la Roche so

properly calls "*inflammation d'entrailles*." The same is equally true of the stomach, the liver, the spleen, diaphragm, &c., so that if the whole peritoneum is inflamed, we have superficies-inflammation of all that is contained within the whole belly.

258. Under such an aspect as this, the peritoneal inflammations acquire an immense importance; for no one can deny that the body must inevitably perish, in which all the abdominal viscera are wrapt in the blaze of a rapid inflammation.

259. It is good, too, to inculcate such a doctrine, because if it is a correct and true one, then it will be believed that all such attacks may have their beginning in a small circumscribed area or areas, and we shall be convinced that to cure them at all, we must begin at once, and act with that degree of energy that appears indispensable, if we would crush the monster at a single blow. This doctrine ought to render us vigilant in our duty, giving timely warning and instruction to interested parties, in order that we may be promptly summoned to witness, if possible, the very first manifestations of the incipient mischiefs. Many of the brethren are, I fear, blamable on this point. Gordon's great merit, that for which he deserves a statue in England, consists in his strong, earnest exhortations to his brethren to be very vigilant on this point, declaring that if they should allow a certain number of hours fatally to elapse, there is little hope, afterwards, of rescue for the unhappy woman. You and I and every physician, owe to Gordon an repayable debt. He was a great benefactor to all society; and if the study of his book will make you be watchful, your whole clientage will in like manner fall in his debt.

260. I hope by this time, you comprehend the meaning of the world's word *child bed fever*, and know that it comprises a group of diverse inflammations within the belly, in women pregnant, or recently delivered; and that it may be metritis, pure and simple, with or without tendency to putrescency of the organ; phlebitis of the womb, tending to rapid ramollescence or putrescency; or else phlebitis of veins external to the organ; ovaritis, with peritonitis, &c. &c.; peritonitis, pure and simple, expanding its area or areas so as to comprise within them many, or all the viscera of the belly, which become superficially or more deeply inflamed; or all the above, combined in the same individual case.

261. These are the disorders you are to combat, and these the mischiefs, which, if you can cure or prevent them, will cause you to cure or prevent childbed fevers. In my next letter, I shall inquire how it is that these diseases prove so destructive, and point out the course they take.

C. D. M.

LETTER IX.

DESCRIPTION OF A CASE OF PERITONITIS IN A WOMAN RECENTLY CONFINED.

262. LET us to-day study the course and progress of a child-bed fever that consists in a peritonitis pure and simple.

263. As soon as an area of phlogosis becomes fairly established on some part of the intro-pelvic peritoneum, the nervous system of the woman, perceiving the shock of the irritation, gives expression to its sensibility thereto by some pain or soreness of the part, by an increased frequency of the pulsations, and by a state denominated rigor or chills, soon followed by a febrile paroxysm.

264. It is not a very common event, for any febrile paroxysm whatsoever, to be formed without an initiative stage of coldness or rigor. It is certainly very rare for constitutional reaction against a peritonitis, or other considerable inflammation, to begin without this ushering and declaratory phenomenon.

265. The woman may, or may not have discovered the pain, before the beginning of the rigor. If the incipient area of the phlegmasia have been seated in a part undisturbed by motions of the trunk or limbs, or if it have commenced during a deep sleep, it is probable that the rigor will be the first perceived phenomenon, to be immediately followed by the pain; but I much question whether the pain is not generally the first perceived affection, though it is often wholly misinterpreted and misunderstood.

266. I have met with instances, when the woman knew nothing and felt nothing of indisposition, until, after turning in bed, or after coughing, or making some movement; whereupon she has at once begun to perceive that she was in violent pain, and has instantly afterwards begun to shiver or shudder with the rigor; the pulse very soon acquiring a great frequency; the

radial artery being at first very small and contracted; but, as soon as the hot stage has fairly set in, the pulse acquiring a considerable volume, and beating with a smart, or even violent stroke; and this particularly in the sporadic or accidental forms of the malady. On the other hand, I have seen many persons, whose first sign was in the chill or rigor. There are instances, in which, at first, the pulse is really synochal, but in many of them it never attains to a great degree of either strength or volume. It is amazing to observe with what rapidity the areas of inflammation do expand in certain women. I am sure that, in many of them, only a few moments are required to carry the red blush of the peritonitis throughout the whole pelvic cavity, and afterwards lift it rapidly, and far above the plane of the strait, and on the cœcum, mesocolon, and whole colon and epiploon. But, in such case, the inflammation is no longer intro-pelvic; it has become abdominal, and is now typhlitis, colonitis, enteritis, &c. &c. &c., as far as it expands and embraces within its range the organs contained within the belly.

The pain in peritonitis is anguish; the least movement aggravates it; she must take a position upon the back; she must draw her knees upwards to flex the thighs, and let the abdomen be as supple and relaxed as possible; for the woman cannot endure to be touched there, nor can she bear the weight of her clothing; she dares not even cough nor sneeze; and gently to draw in a full breath, she cannot be persuaded to do, lest it should increase her distress.

267. The whole nervous mass of the woman is in a state of alarm or excitement, for it is continually receiving pungent pathogenic impressions, and every nerve that reconducts the reflex influences to their proper destination within the organs, carries with it a stream of neurosity that may be compared with the fires of the electro-galvanic current. All the organs thus become excited, and now we have this fever, which the world will call childbed fever; not before, but after the establishment of the phlegmasia.

268. As soon as the bowel begins to participate in the inflammatory attack, it at once ceases to resist any tendency to inflate its tube; and very early in the case, even as early as within 20

or 30 minutes, the belly becomes tumid and sonorous on percussion, the tympany of childbed fever being so soon formed. It is a very singular distinction that you should make between ordinary peritonitis, as in males and non-pregnant women, and that of lying-in women, that in the former, the disease is accompanied by an extraordinary rigidity of the abdominal muscles, the belly being, for the most part, hard as a deal board, and strongly contracted or drawn inwards; while in childbed fever, the ballooning is an almost invariably present symptom. Do not be led to believe that the tympanitis is caused by gases in the peritoneal sac, as some have pretended; neither imagine, with Gasc. 207, that *Dans la fièvre puerpérale, ce météorisme de l'abdomen, est dû en général au boursoufflement du tissu cellulaire voisin de l'organe affectée; la présence des gaz dans les intestins y contribue pour fort peu de chose*—which, I think, is medical nonsense.

269. This tympany or ballooning in the belly increases rapidly, insomuch that, between the onset and your visit, if you are dilatory or late, the abdomen shall have become almost as large as it was before the labor began; nay, even much larger.

270. The chill may continue for a quarter or half an hour, and is even observed in some to last for two whole hours, before the hot stage of the paroxysm becomes fully established; for the whole case is a paroxysm, with its consequences.

271. The pulse usually beats 120 to 140 times, and in some cases even 160 times a minute. The face assumes a brick or modena-red color. There is great heat, and intolerable thirst. The head aches, and the countenance assumes an expression not to be described, but needs to be painted or seen, to know it. It wears an expression of fright and distress. The French authors call it *figure tirée*, by which they mean that the skin of the forehead is tightened, or drawn upwards, giving rise to the singular physiognomical expression. If the lochia have been before abundant, they are now wholly or partly suspended. The breasts either remain flaccid and without milk, or, if the secretion had before existed, it soon lessens and makes haste to disappear. The skin is hot and dry; the tongue, broad and flat, is covered with a whitish induitus, that looks as if the woman had been recently drinking milk—she has a milky looking tongue.

272. As the areas of phlogosis expand wider and further from their original centres, greater and still more important mischiefs continue to be perpetrated within the abdomen, and the nervous mass reels and trembles under the load of irritations it can neither overcome, nor even resist. The sanguiferous and nervous reactions that flow out from their great centres, serve, not to cure, but only to augment the mischief, and draw on the ultimate ruin both of the organs and the centres, which fail, and faint, and die beneath the exciting power of the topical lesions.

273. I saw a young creature here, a few years ago, who was confined in the night after an easy labor. The physician left her well, and went to his house. He was recalled early the next morning to find her in the state above described. I visited her with him in the forenoon. The pulse could scarcely be counted for rapidity; the belly was bigger than that of a woman with twins, sonorous as a tambourine, on being percussed, and sore as the apple of an eye. The breathing was short and imperfect and singultiform, because the diaphragm could not go downwards in inspiration, without crushing beneath it, in descending, a whole inflamed mass of bowels and other abdominal contents, and so, giving intolerable pain. It was impossible to procure any alvine evacuation, or to do anything else that might save or even relieve her; she died within twenty-four hours after her child was born.

274. In many a case have I been astonished at the rapidity with which the belly became thus tumid; and so terrible have seemed to me the influences of such a condition upon the vital functions, that I can never perceive a speedy and great development of the tympanitis, but as an unmistakable sign of pressing danger to the patient. I except only certain rare examples of enormous hysterical tympanitis following parturition, but that are wholly unconnected with any inflammation of the contents of the pelvis or the belly. I now ask you how it could be otherwise than as I have herein above explained? Let us inquire. Say that the pulse was 180, and that the heart, in health, beats 70 times in a minute; one hundred and eighty beats per minute is 10,800 beats per hour; seventy beats per minute is 4,200 beats per hour. The heart, then, beat one hundred and eleven times

per minute more than is normal, or 6,660 times per hour more than it ought to have beat, and this in 12 hours would give 79,920 extra beats of the heart, or extra uses of the systolic force. One might suppose that this, alone, should suffice to destroy the woman, and that the heart, tired to death by so rapid a flight, would succumb under the over-stimulation and consequent exhaustion that drove it to such excesses of activity.

275. There are three great vital functions: those of the brain and nervous system; those of the heart, or circulation; those of the lungs, or respiration, which serves as the oxygenating function for the whole economy. Now, when the belly has become enormously distended by gases contained within the tractus of inflamed intestine, the diaphragm will not go down, to crush beneath its contractions the agonized mass of inflamed intestine below it. The woman tries to breathe by the ribs, with a little assistance from the diaphragm; the respiration is therefore imperfect and incompetent to its ends. She does not take in a full aspiration of some 18 or 20 cubic inches of atmosphere for each breath she draws. If you look attentively, and observe the rate and manner of her breathings, you will see she does not breathe enough and does not get breath enough, even though she breathe fast and quick; you will see that instead of 20, she takes only 8, or 10, or 12 cubic inches at each breath; and out of that small quantity, consisting of azote 79 and oxygen 21, she can only appropriate a lessened portion of the 21 per cent. of oxygen of the air at each aspiration. But in health, she requires to get 18 or 20 cubic inches with each act of breathing, in order normally to oxygenate the blood in her lungs; or, in other words, to oxygenate her system. Let us say that a healthy adult will breathe 16 times a minute, and with each inspiration take 20 cubic inches of air. Sixteen times 20 is equal to 320 inspirations per minute, which is what is required in the normal exercise of the respiratory force. Reduce the quantity to 10 inches at each inspiration, and then 20 times is equal to 200 only, so that your patient acquires 120 inches per minute less than is normal, which equals 7,200 cubic inches per hour less than she requires. But with so great a reduction as this, she would die, unless she should repeat the breathings more rapidly; and so, in fact, she does. But the respirations that ought to be only 16, rise to be 32 or 50 per minute, and that rate

is a most exhausting rate for any human being, and must soon end in developing a state of cyanosis or asphyxiation.

276. Is it not easy to discern that with such incompetent oxygenation as this, she must rapidly succumb? that all the functions must give way, and be at last wholly overthrown and ruined? I declare to you, that whenever I see one of my patients in child-bed fever afflicted with enormous tympanitis, I not only look on it as an evidence of great inflammation within the belly, but as almost sure to kill her, by cutting off, in the manner above explained, the due supply of oxygen for the blood.

I have on many occasions of women dying with peritonitis, watched with excited interest the gradual unfolding of this rôle of appearances, manifested at first, in a bluish areola under the eyes and about the upper lip, and a mortal and peculiar pallor of the face; then the first perceived blueness of the finger-nails, followed by the cyanotic tint of the hands and feet; and it seems to me that you ought to consider that all such women when sinking, are sinking, not merely under a load of intolerable constitutional irritations, but they are also dying from the lessening power of the blood in the lungs to take out of the atmosphere the proper ratio of oxygen, because the diaphragm will not do its office aright. They are dying with a real cyanosis.

If the blood be driven into the pulmonary artery and its branches by 180 instead of only 70 pulsations a minute, and if the breathings be repeated 50 or 70 instead of only 16 times per minute, the greatest evils must arise as to both those important functions, the circulation and the aeration of the blood. If the oxygen is thus not transported in due amount to the encephalon, the heart fails in its office, a failure which draws on, as an inevitable consequence, the failure of the respiration; and when heart, brain, and lungs together refuse to do their work, the patient dies. The triple union between the heart, brain, and lungs, or the circulation, innervation, and aeration, as explained by Bichat, can never be broken up without drawing after it the fall of the whole.

Hence, a woman sinking under the overwhelming pain of a constantly progressive peritonitis, always dies in a cyanotic state, brought about long before she becomes really moribund, by means of this Tympanitic interference with the respiratory processes. This cyanotic condition tends mightily to contravene the

beneficent influence of all our remedies; wherefore, I say that a prime object of a practitioner's attention should always be, the subduction of the meteorismus. I think that only a little clinical experience and observation could be required to make this matter perfectly clear to you. You would note the number and fulness of each act of breathing, which you would find to be singultiform, and very short; and though the breathings might be more frequently repeated than is natural, you should find that she would not breathe enough or get the needful quantity of air, however much she should strive for it, and so, the blood becoming less and less reddened by oxygen, would develop in the encephalon less and less of the neurosity. Such an effect allowing the heart and the lungs to grow weaker and weaker, she would die at last, in a good measure at least, because she had become cyanosed.

277. This tympanitis becomes in not infrequent instances, the positive cause of a fatal termination of the disease in another way. The cavity of the abdomen, which possesses only certain dimensions, becomes too limited in capacity for containing the inflating bowel without angulating it at the returns of the convolutions. I have, upon dissection, after great tympanites, observed that the large intestine was so distended, that it was not possible for its return to take place, without acute angulation, at the point where the bowel should turn to cross the abdomen again.

278. In a young lady's body, I found upon opening the abdomen, which previous to her death had been greatly inflated, three parallel rows, each as large as a stout man's arm, lying obliquely across the belly and extending as far as the abdominal walls would permit. These parallel tracts of the colon returned by such sharp angles that the bowel was effectually obstructed. If you will take a sheet of paper, and roll it up, making of it a hollow cylinder, say an inch or an inch and a half in diameter, it might serve to represent a cylinder of intestine; next, bend it at a sharp angle, and you shall find that the tube is completely obstructed by the sudden sharp angulation of it at the return, so that you cannot force air through it, by blowing, even with the greatest possible force, in one of the open ends of the roll. This is exactly what happens in many cases of childbed fever, when the tympanitis becomes very great, and I am sure I have

seen several women die speedily, because the caliber of the intestinal tube was thus completely closed. This was the case with the young woman I spoke of in sec. 241, which I invite you again to peruse. It was also the cause of death in a charming woman, whom I lost here some five years ago. You may well suppose that such a cutting off of the cylinder of the bowel, would have the same tendency to bring on dissolution, as would a strangulation of the gut by the abdominal ring, or Poupart's ligament. Such people do suffer from a real *iliaca passio*, and show it by the vomiting of feces, as I have witnessed in the cases.

279. I repeat, therefore, that I am always greatly alarmed for my patient, when I discover that the tympany is very considerable, and that I am then always fearful that no aperient or purgative medicines can possibly succeed in opening the bowels. So I have found it in practice; and so, I apprehend, will you find it, and be able to explain the impossibility of obtaining any alvine passages, on the hypothesis, or rather the assurance, that the bowel is shut up by its angulation. Take care, therefore, to adopt prudent measures for the obviation of so dangerous, and even fatal an accident.

280. I have stretched the alimentary canal of a slaughtered sheep upon the ground, and found it to be sixty feet in length. In a gray squirrel, I have measured one of sixteen feet, and in the human adult, I have found it fully forty-five feet long when stretched across a long anatomical theatre. If you should, with an enterotome, lay the whole tractus open upon its mesenteric line, and then lay the intestines out like a broad flat ribbon, you would probably find it four inches in width, by forty feet, or four hundred and eighty inches, in length. The square of this surface would therefore be 1,920 inches, or about thirteen square feet. Add to this sum the rest of the peritoneal reflections, and you have some sixteen or seventeen square feet of serous surface, within the belly, the whole of which may be at the same time in a state of inflammation.

281. Now you know that a man suffers no little constitutional irritation, if he have an erysipelatous surface of six or eight inches square. Such an erysipelas would create much fever, and perturbation of the functions. From this it seems probable there

are few women who could endure the expansion of the primary areas of a peritonitis, until they should come to involve the whole thirteen or seventeen superficial feet of the serous membrane: indeed, most patients would and do sink before the area could become so great. Yet I doubt not I have examined some bodies, after death, in which the whole peritoneum had been inflamed before the last sigh of the expiring victim. How can you, after this representation, wonder, when you observe the rapid overthrow of all the nervous power, under such a vast inflammation as this; or why should you go aside to seek for some supposed typhus, or some unimaginable poison, and bring it to explain why the woman becomes so weak and exanimate? Take my word for it, that in so dreadful a disease as this, there is, in general, no hope of any useful interference by the physician, or his drugs, except he begin early, and, by the employment of a prompt, sufficient, antiphlogistic remedy cut the disease off at a blow; for if he should be either dilatory in his ministrations, or weak in his processes, but a few hours would elapse ere he should discover the signs of that putridity, or that vitiation of the fluids, which, instead of being causes are only inevitable consequences of the disorder.

282. In expanding peritoneal inflammations, not only does the bowel soon begin to yield to the distending force of the gases extricated within it; but the stomach itself grows sick, and shows it, first, by a sense of weakness, then by eructations, and at length by vomitings. The liver also soon begins to feel the disordering influences of the obstructed meseraic circulation; for, as the whole of the blood that flows in the arteries, capillaries, and veins of the digestive tube, and the mesentery and mesocolon, comes out from the aortic torrent through the coeliac, and the two mesenterics; so the whole of it, except what is employed and used up in the organic nutrition, and in secretions, must be gathered into one stream in the portal vein, thence to pass into and be distributed within the liver. You cannot thus have an idea of a great peritonitis, without at the same time inferring trouble in the meseraic and hepato-portal circulations. But, when the peritonitis has mounted up and spread upon the liver itself, inflaming its serous coat, that organ becomes diseased; and so, in fact, as to every viscus within the belly. They all become inflamed as to their serous coating, and therefore add to the confusion of irrita-

tive perception or sensibility, under which the nervous mass reels and dies. The nerve centres give up the struggle; they can no longer control the life of their subject-organs, for the nervous mass is now like a drunken man; it lies down in its intoxication or exhaustion, and sleeps until it dies, while all its subjects perish at the same hour.

283. In the progress of these events, when the inflammatory lesions have affected the peritoneal surface of the stomach itself, we might consider that organ as beginning to die; the very first signs of which are seen in an occasional eructation of air, next of a small quantity of colorless fluid, then a verditer-colored mucus and bile, which grows greener and darker, until, at last, great stores of granular matter, suspended in thick greenish secretions, exactly like the black vomit of yellow fever, show that the work of mischief is completed, and the expiring woman dies perhaps while rejecting from her stomach the last drops of cordial, that have been urgently tendered to her by the hand of despairing charity, still wishing friendship, or broken-hearted love. There is scarcely to be met with in the whole painful mission of the physician, a scene more heart-rending. He stands with folded arms, the unwilling witness of a ruin, no wisdom, sagacity, or energy can now prevent, or even oppose. My dear young friends, I earnestly pray God to spare you from witnessing many such sights. If you have hearts in the least sensible, they must be wrung whenever such occasions shall hereafter arise for you.

C. D. M.

LETTER X.

METRO-PHLEBITIS.

284. HAVING exposed to you my opinions concerning the peritonitic form of that group of inflammations denominated childbed fever, I wish now to call your attention more particularly to another and most important one, I mean the phlebitic form of the malady, though I have already laid before you M. Dance's views.

285. M. A. C. Baudelocque's prize-essay, entitled *Traité de la Péritonite Puerpérale*, was adjudged and crowned May 24, 1829; and I beg you to note that it is a treatise on puerperal peritonitis, and that it is not a work on those metro-phlebitic forms, which, I expect, you will henceforth consider as the most serious and difficult of the group of maladies collected under the general name of childbed fevers. The imprint of Baudelocque's work bears the date of 1830, the year in which Tonnellé's papers appeared; so that its author had not made that progress in the knowledge of the pathology which became general after the appearance of Tonnellé's essays.

286. Although I greatly admire Dr. Baudelocque's Treatise, I may be allowed to observe that he begins it with a grave mistake, in saying that "the puerperal peritonitis, which was long confounded with other diseases under the names of fevers of lying-in women, puerperal fever, miliary fever, *depôts lacteux*, inflammation of the bowels, inflammation of the womb, inflammation of the lower belly, &c., has always suffered under the yoke of the reigning doctrines in Medicine," etc. etc.

287. I say that the author starts wrong, for he assumes that peritonitis is the disease, and that metritis, &c., are the complications; whereas, it is now conceded that the most deadly form of childbed fever is that in which a rapid decomposition of the uterus

takes place, under an inflammation which reduces the whole, or large portions of the texture to the state called *putrescentia uteri*, or putrescency of the womb; and that, without the least implication of the peritoneum in the malady. Baudelocque expressly states at page 161, where he begins to treat of the section of "complications," that puerperal peritonitis does not always exist alone, but frequently is complicated with another disorder, especially in hospitals; and, what is singular, he insists that the most common complication is with pleurisy.

288. This is what a distinguished writer says, and I have no objection to it except that it is by no means true, and that any one of my Students, who should read the book, would be in danger of falling astray from the truth, in childbed fever pathology, by confiding in that author's opinion.

In a former paragraph, sec. 287, I adverted to the opinion expressed by the younger Baudelocque concerning the frequent complication of pulmonary inflammation with childbed fever, and it is true that such complications are sometimes to be met with, though my own clinical experience furnishes me with few such examples.

If we assume that pus excreted upon the surfaces of veins in phlebitis may be transported by the torrent of circulating blood that, passing through the vessels, washes away and so transports the pus-corpuscles, we might infer that those corpuscles might be detained in some portion of the capillary vessels of the lungs, and, there arrested, give rise thus to inflammation. Perhaps this is really the case, and so furnishes a good rationale of the occurrence of the pneumonia. Any person, however, who may have had opportunities to observe the results of phlebitis as seen in the *post-mortem* examination of the cases, will have observed that not only is much pus to be detected in the hollows of the tubes, but considerable quantities also of fibrina, or masses of exudation-corpuscles, that are as liable to become detached as the pus-grains themselves, and so carried forwards towards the heart. Suppose, for example, a portion of solid fibrin as big as a pea to be washed forwards and conveyed into the right ventricle, to be thence driven into the pulmonary artery, and so impelled into a lung. It is in vain to deny that the concrescible elements of the blood do, on occasions, become concrete masses of various sizes, and that they

really are to be floated in the venous currents towards the heart and into the lungs. No person can deny this; but the question must come up as to what becomes of these concrete masses—when will they be arrested—since they cannot traverse the fine capillaries of the pulmonary arteries. The answer must be that they do stop in the pulmonary vessels, and serve, like the tampon, to fill up and close the channel into which they are driven to clog and obstruct it.

What say you, then? Are not many of the lobular, and even lobar pneumonias thus observed to attend upon attacks of childbed fever, to be reasonably attributed to the lodgement of concrescible materials of the blood within the delicate pulmonic capillaries? and by their intrusion, pressure, presence, and obstructing power, there awakening the trains of pneumonic inflammation so much complained of as accidents in puerperal fevers, that some authors, and among them Prof. Ritgen, are disposed to regard the pneumonia as one of the common forms of the disease? for he uses the following words, which I here cite just as they are to be found at p. 18 of *Gemeinsame Deutsche Zeitschrift für Geburtskunde*, Band vi. Heft 1: “Steht aber dasjenige, was über das Wesen des Kindbettfiebers aus der Natur des Zustandes Neuentbundener, und aus den erfahrungsgemässen Erscheinungen des Kindbettfiebers entwickelt worden ist, richtig; so muss einleuchten, dass hier keineswegs immer von einer Bauchfellentzündung und, wo diese zugegen ist, keineswegs immer von ihr allein, die Rede seyn könne, sondern dass alle Häute und selbst alle in sie gehüllten Gebilde der Unterleibshöhle, sondern auch die der Kopf- und Brusthöhle, somit alle Eingeweide der drei Haupthöhlen ohne Ausnahme, beim Kindbettfieber entzündet sein können.”

Now it seems to me very natural that, during the wild fury of the blood, in these inflammation-fevers, the lungs, or any of the contents of the three great cavities should occasionally be found or become seats of inflammation. Yet I am far more inclined to adopt the rationale that they are pure results of the mischievous presence of small masses of solidified concrescible elements of the blood, driven into and detained within the capillaries of the organs. These incidents are far more likely to accompany the phlebotic than the peritonitic forms.

289. There is no occasion for me to go over again an exposition of my views as to the nature of the true bloodvessel, or to reiterate the deep conviction I have on the nature of its power and office as the blood-membrane—the tissue which is directly concerned in the acts of the hæmatisis; I beg you to refer to my sections for what I consider true and useful on that point.

290. Phlebitis, which I have often treated, both as uterine phlebitis, crural phlebitis, and phlebitis traumatica, is a most insidious disorder, making considerable progress, before either the woman's feelings as to disease are alarmed, or the nervous sensibility aroused; indeed, it may exist, and prove fatal, without giving pain, almost. See the following case from my notes in 1831:—

Thursday, March 17, 1831; Mrs. F. A. J.'s second pregnancy, in the seventh month; attacked with uterine hemorrhage while walking in the street, with which she became greatly alarmed. Labor coming on soon, she was delivered at one o'clock this morning. The infant, which was alive, came into the world with a cyanosed face, and grew more and more livid, and, after a short time, expired. The young lady was excessively nervous, and I, on that account, directed the nurse not to allow her to see the child, because its face, as black as the deepest colored negro's, might dangerously shock her in her weak state. The wretched woman promised obedience; but when the young lady afterwards expressed a strong desire to look at it, she consented, and having dressed it all in white, and laid it in the little coffin, she brought it to my patient's side, saying: "There, deary, there's your poor little baby." My patient, who at the moment seemed as well as any woman can be, under the circumstances, rose upon her elbow, and looking fixedly at the object for some time, turned her eyes upon the nurse, and with a wild stare and in a sort of shriek, exclaimed: "That my child! Do you dare to say I have given birth to a black child? I, I give birth to a negro!" Her excitement was terrible. This was on Friday, the 18th. At twelve o'clock she got a dose of castor-oil, that operated once slightly at five o'clock, after which she had a chill, and violent pain in the abdomen. I arrived again at seven o'clock, and when I reached the chamber she began to laugh merrily, and said she was quite

well now. "Let me feel your hand, if you please, Mrs. J." "No I sha'nt; what do you want to feel my hand for? I saw the child, Doctor. I never before saw a coffin in our family. How black the baby was! But it wasn't a black child, Doctor, was it? I didn't see my brother—do you know my brother was drowned in Schuylkill? But I didn't see him, I only went to the stair-head—I couldn't go any further." And so she rambled most volubly, stopping, every now and then, to complain of pain "all round" her. The skin was hot, the tongue clean, the lochia abundant, uterus firm, pulse 130, belly tender to the touch. Sinapisms to legs; dose of oil; stupes with hot brandy. After the sinapisms the delirium vanished; oil operated freely, once. Effervescing draughts. At twelve o'clock P. M. I found her in excruciating pain of the womb, which was closely condensed. No tenderness, except of the womb itself. Camphor-emulsion, with laudanum. On Saturday, 19th, all the signs of acute peritonitis were present, and I bled her to fainting. I bled her again on Sunday, the 20th. On Monday, 21st, great tympanitis. Tuesday, 22d, pulse 140; milky tongue; tender and distended belly. I sat up with her until half-past three o'clock this morning, during which time she was dreadfully agitated about dying. Prayers; hysterical excitement; then cataphora, with rigid locked jaws, after which seemed much better; meteorism gone; pulse 150; profuse warm sweats; seven to ten alvine dejections. In about two hours the hysteroid symptoms reappeared; bad singultiform respiration; pumping up colorless liquids from the stomach. Wednesday, 23d. Bad night; a violent fit of screaming at four o'clock this morning, with hysterical asthma or respiratory convulsion; pulse 145, fuller and more vigorous; face wears a good expression; skin warm and moist, not hot; dorsal decubitus; belly not in the least swelled. Has *retained* three enemata, one of Spanish soap, and two with assafetida, making twenty fluid-ounces in all. She is quite sensible. *I now think she will recover.* Thursday, 24th. She was in a hysteroid state all day. Not much tenderness, except immediately over the womb, whose fundus seemed to be almost as high as the navel: it felt doughy and inelastic. The pulse grew more and more frequent, and she died, without struggles, and in her full senses, at three o'clock this morning, having been ill for seven days. I did not examine the

remains. I treated this case very inefficiently, twenty-three years ago—certainly, not as I would now treat a similar case. My error was, in having considered the early attack as hysterical, and the first severe pains as after-pains. The truth is, an area of inflammation, rapidly becoming phlegmonous, was at the moment being laid in the womb, and which afterwards became membranous, or peritonitic. The latter I did cure, but I did not cure the former, and it passed into phlebitis, with pyæmic intoxication, evinced by the symptoms I have related. I was also greatly mistaken and misled, indeed, by the favorable symptoms observed on the Wednesday, which flattered me my patient should recover, and so only rendered my disappointment more bitter.

I pray you consider this case, and let it prompt you to act more wisely than I did, and also let it warn you not to allow any accouchée to receive a fearful shock like that this poor girl had, when the blackened visage of her dead baby started the wild idea that her character was blasted—she knew not how.

291. Changes in the vital state of the *membrana communis*, going on unperceived, are often found, when their existence becomes revealed, to have reached, already, an incurable stage; and it is, in fact, only when the inflammation of the vessel has really passed downwards into the basement tissue upon which the *membrana communis* reposes, that pain begins to be felt and alarm taken. This is pre-eminently the case in the instance of crural phlebitis, or milk-leg as it is vulgarly called. In many an instance I have been the first to detect its existence for the patient, who did not know, nor even suspect, that anything was wrong as to the limb, until, by compressing the soleus and gastrocnemius muscles against the tibia, I gave her sharp pain, and then pointed out to her the swollen and painful tractus of crural vein below Poupart's ligament, or the painful and hardened vessel distinguishable above the pubis, and along the brim of the pelvis.

292. In like manner I have seen phlebitis of the median vein from venesection, which the individual knew nothing of until I sought for the inflamed tractus in his arm, being led to suspect phlebitis somewhere, on account of the constitutional symptoms; thus detecting an inflammation that soon afterwards destroyed his life by purulent infection of the blood.

293. Professor Jörg, *op. cit.* 665, treats—*Von der Schwäche und der Putrescenz der Gebärmutter im Wochenbette*—of Weakness and Putrescency of the Womb in Childbed. He does not refer to the mere muscular debility that leads to hemorrhage, &c., but seems to suppose a profound debility of the whole life of the organ, that allows the tissues to undergo decomposition. In his § 710, he remarks that if the weakness of the womb attains to a certain degree, and is, at the same time, connected with loss of general strength in the system and diminished assimilation-power, the uterus passes into the condition denominated putrescency, which commences in the inner surface of the cervix, without any antecedent inflammation, and extends outwards, more or less involving the entire structure. For my part, I cannot conceive of such a state of a living organ, as that we observe in putrescentia uteri, being brought about by any other causes than the most furious inflammation. Inflammation is a bursting process, whenever it is rapid or acute; while slow or chronical inflammatory processes result in the evolution of new forms and substances, as is finely explained by M. Serres, at p. 102 of his *Récherches d'Anatomie Transcendente et Pathologique*. Already, in sect. 252, have I expressed the opinion that those rapid decompositions of the womb called putrescentia uteri, are nothing else than a melting down of the texture under sudden suppurations of the capillary Endangium of the whole organ; an opinion which, I apprehend, will not meet the sanction of my readers in general—a circumstance I regret the more, because, as it affords the only possible solution of the problem why such putrescentia occurs with such hasty processes, I shall conceive that they do not deem of it as they ought. Look at p. 326 of *A Pract. Treat. on the Dis. of the Uterus and its Appendages*, by Mad. Boivin and M. A. Dugès; London, 1834, 2 vols., 8vo., for what the authors say on this subject. But I shall quote part of the paragraph: “Notwithstanding the doubts we have raised, we shall perhaps be obliged to acknowledge more distinctly hereafter, that partial softening is only the earliest stage of phlegmonous metritis, infiltration the second, and abscess the third. We are induced to think this is the case, from the circumstance of the softening being, generally, peculiar to recent and severe metro-peritonitis, and abscess to the same affection, when

approaching to, or having reached the chronic form," &c. &c. Dr. Robert Lee adopts nearly the same views.

294. I wish you to consider the womb as an organ supplied with the materials of its sanguine circulation, principally by means of the two uterine arteries, yet in part also, by inosculations with branches of the ovaries; these together are the only sources of supply of blood for the uterus. Torrents of blood are thus driven within the texture of the womb, through those afferent ducts of the blood. This mass of sanguine fluid passes off through the efferent or uterine and ovaric veins. Now, whenever, in a case of uterine phlebitis, and under an excited state of the circulation, large quantities of blood are propelled by the heart into the capillaries of the womb, it must happen that, with a considerable degree of *phlebitis uterina*, those inflamed, and, so, obstructed efferent vessels shall not be able to deliver the fluid as freely as they can usually do it, and therefore the womb can with difficulty be relieved from the engorged and inflamed condition in which it is thus placed.

295. Whosoever is familiar with the course of a case of crural phlebitis, must have observed the generally engorged state of the whole lower extremity, and the difficulty, nay, sometimes the utter impossibility of finally curing the swelling, owing to the ruin, by inflammation, of the great crural vein, which is the principal efferent duct for the blood of the whole leg. Certainly, all experienced practitioners have met with instances, in which the engorgement and enlargement of the leg never could be got rid of in thirty years of the subsequent life of the patient, because the outlet by the efferent vein had been partially destroyed during the phlebitis. Dr. Robert Lee tells you of cases in which, by a crural phlebitis, all traces of the internal iliac vein became lost, like an African stream, that sinks in the sands of a desert. See his account of Mrs. Jones's case at p. 294, *Treatise on Puerp. Fev. and Crural Phleb.*, Barrington & Haswell's edit. His words are: "On tracing upwards the obliterated vein, that portion which lies above Poupart's ligament was observed to become gradually smaller, so that, in the situation of the common iliac, it was lost in the surrounding cellular membrane, and no traces of its entrance into the vena cava were discernable." How could such a limb perfectly recover? and is it not a fact that a bad crural

phlebitis, though recovered from, as to the patient's general health, generally leaves the extremity liable to swell, for months or years afterwards, upon any excess of exercise with it, since the blood conveyed to it by the femoral artery finds no easy return by the ruined veins?

296. These observations may serve to set before you, with clearness, the idea that acute uterine phlebitis must be ever a dangerous disorder, because the power of delivering the injected blood is partially cut off, and, therefore, the inflammation within the tissue is not likely to recover by what the authors call resolution of inflammation, but strongly tends to effusion and suppuration.

297. M. Tonnellé has already shown you the ratio of cases of pure metritis and phlebitis to cases of peritonitis; see my sect. 226: and his statements should convince you that it may even happen that a woman shall die with childbed fever, consisting, essentially, in inflammation of the uterine veins alone. Look at Wm. Hunter's famous plates of the gravid uterus, to see what is the magnitude and number of those immense vessels, that he pictures as pursuing their tortuous courses on the exterior of the organ, and observe the effigies of their enormous openings on the cut surfaces. Those plates alone are sufficient to give you the needful information as to the magnitude and copiousness of the vessels, if, indeed, you have not had the melancholy privilege of seeing them upon the dead subject; an opportunity that is certainly not often to be had; for few men can say, with Noortuyck, "*hocce opimum spoliū mactus*," for it happens to few to inspect a recent uterus at term.

298. That a woman, under these circumstances, should die, is not to be wondered at; nor even that she should quickly perish; for the whole mass of the blood has become infected; that is to say, the purulent corpuscles, formed by the inflamed inner membrane of the veins and sinuses of the womb, are washed, by the flowing blood, forwards into the heart, and thence dispersed throughout the system. Blood, containing a considerable quantity of pus, is singularly unapt for the purposes to which, when healthy, it is destined. No sooner does it become somewhat charged with these pernicious corpuscles than the character of the fever undergoes a great and peculiar change. Some authors refer the typhus character of the constitutional phenomena entirely to its presence

in the blood, and, perhaps, with some show of justice; for one cannot conceive of the transit of such corpuscles, along the capillary routes within the encephalon, in the spinal cord, and in and about the great nervous ganglions and plexuses of the sympathetic system, without concluding that the greatest derangement must ensue as to their vital states and the coincident forces. I am not, however, prepared to attribute the typhous characteristic of child-bed fever to purulent infection alone, since I am convinced that many patients, dying with peritonitis pure and simple, do exhibit those typhoidal characteristics during the last stages, though there is not any reason to infer that purulent infection has taken place in any mere peritonitic form of our disease.

299. Some persons appear to think that the pus-corpuscles, that infect the blood in these grave cases, get into the stream by being absorbed; and it is even imagined that pus may pass from the hollow of the womb into the patulous orifices of the vessels, and so, infect the mass of the circulating blood.

300. You ought, gentlemen, to consider that pus cannot be absorbed, because all absorptions and resorptions take place by endosmose through diaphragms that shut all absorbing mouths. If you answer me by saying that pus, when deposited within the walls of an abscess, is known to be sometimes removed by absorption, I have only to make the rejoinder that this process cannot take place save where a fatty degeneration of the material first allows of the absorption. I repeat it, absorption of corpuscles of pus, or of blood, as such, is impossible. They must previously undergo a change in order to let them pass through the absorbing diaphragms. If it were true, which I believe it is not, that the mouths of absorbents may be open and pervious while bathed in purulent matter, then you could, with some semblance of correctness, say that the pus could be, and is, directly and in substance absorbed; but if the orifice of every absorbent is shut by a membranous diaphragm, through which endosmose of the absorbed fluid takes place, then granules or corpuscles of pus could not, in any case, be absorbed; for it is as easy for you to conceive of a charge of mustard-seed shot or buckshot passing by endosmose within through these diaphragms, as of a drop of pus doing so, since the corpuscles of it are solid substances incapable of transfer

by endosmose. Every instance, therefore, of purulent infection of the blood, has, as a condition of its existence, the inevitable complication of a phlebitis somewhere that has passed on to the pyogenic stage. That is, the inflamed endangium has allowed the formation of a purulent deposit to take place on its surface. This being first done, the corpuscles of pus or the entire purulent material being carried forward towards the heart in the veins, the fluid becomes vitiated or infected with pus, which is pyæmia.

301. It will require no long years, nor any excessive application to obstetric practice, to afford you opportunities to witness the effects of this pyæmia, both upon the physical and intellectual state of the woman; and when once you shall have become somewhat experienced in noting the signs of it, you will have become *ipso facto*, able, with considerable accuracy, to determine the seat of the inflammatory lesion of the veins.

302. I consider it would be a vain thing for any man to try to portray the features of the case with any pretensions to give an accurate delineation of them. It is, perhaps, sufficient to say that in the beginning of the pyæmic affections, there is in the patient an appearance of intoxication, which soon assumes an hysterical or hysteroidal character. The same is, I believe, true of those persons who take doses of arsenic, without speedily dying from the escharotic effects of it, but who are, nevertheless, badly poisoned by it; and we are informed that, in Europe, there are numerous individuals who make use of arsenic, with a view to the same sort of exhilaration that others seek to obtain by alcoholic drinks or opium, keeping themselves in a state of constant slight intoxication.

303. In some of the cases of pyæmia that I have observed, the exhilaration was like that produced by champagne, with alternations of a sort of stupor like a drunken one, changing again to the wildest delirium and the most furious violent mania, the fits again giving place to the gentlest and sweetest tone of the temper and spirits. I have heard such victims singing, like the dying swan, while the departing soul was slowly dissolving the bonds that had mysteriously connected it with its clay tenement; and surely some of these scenes are so touching that their deep traces become ineffaceably impressed upon the memory, and can never

even be recalled without awakening those poignant regrets with which they were originally connected.

My fourth letter contained a rationale of the hæmatosis; and in that letter I particularly invited you to consider the manner in which the hæmatosis of the foetus is carried on by its own blood-membrane, or endangium. Now, so great must be the force of that endangial tissue in its uses of inducting the nerve-force or neurosity, from the solids, into the fluid materials of the blood, that I cannot conceive of mutations in its vital state, as being unattended with changes in the vital state of the blood itself. Inasmuch as all instances of extensive phlebitis must be regarded as passing through different stages or periods in the development of the inflammation, I think we may account for many of the constitutional symptoms that are observable by referring them to progressive stages of the phlebitic disorder. The blood itself cannot but suffer some qualitative change in passing over the surfaces of veins actually inflamed, or even iuflaming, and an experienced discriminating eye may be able to discern, in the physical and physiognomical appearances exhibited, the signs of such change long before the disease has risen to the height of its pyogenic stages. They say that straws show which way the wind blows, and there is a something indescribable in the pulse that portends the coming storm; and it has happened to me more than ouce to prognosticate a violent phlebitis, when no taxis and no clear local signs gave evidence of its forthcoming.

304. In your future cares for persons in childbed fever, I warn you, to look amoug the general signs, as in the pulse, respiration, moral and intellectual state, pain, decubitus, excretions, &c. &c., for the motives that should lead you to the conclusion that what you have before iuterpreted as puerperal peritoutitis only, is, in fact, complicated with uterine phlebitis; and I now venture to aver that, whenever, under these circumstances, you shall observe a combination of hysterical, or intoxicative, or delirious excitement combined with the other and more general phenomena of the case, you ought to suspect that you are to combat one of the most dangerous forms of the whole group of childbed fever inflammations; for a phlebitis will certainly partake of the nature of a phlegmonous inflammation.

305. In the forming stages of the phlegmasia, I conceive it not to be possible, nicely to discriminate between the particular forms the case may assume. Indeed, it might be deemed safest always to suspect some complicity of the metritic or phlebitic phlegmonous forms with every peritoneal or membranous one. M. Tonnellé's statistic of the *post mortem* results cannot be safely taken as positively accurate. I say this, reflecting on the case that I related in a former Letter, sec. 242, one, in which, I doubt not, I did perfectly cure the extensive peritonitis, and then vainly flattered myself that the lady was rescued; whereas, the dissection afterwards disclosed the cause of her death, in that purulent deposit in the uterine veins. I believe that you may hereafter find yourselves as much misled as I was on that occasion; for, if you should not treat your cases aright, you will, in many of them, bring about complete resolution of the peritoneal inflammation, but leave the venous inflammation unresolved, to exert its fatal power some days later. I beg to suggest to you the proper reflections upon the difference there is betwixt a phlegmonous and a membranous inflammation, and that the former must, of necessity, prove more difficult of resolution than the latter; since, in the former, there always is a quantity of exuded or unabsorbed materials to be taken up and carried away, while in membranous inflammations, there being but little absorption required, it is necessary in the treatment, only to impress upon the phlegmasia a resolute tendency; which, being done, all traces of the disease may soon entirely disappear. In September, 1854, I treated a case of rapidly expanding peritonitis in a young girl, sixteen years of age. The loss of blood by venesection and leeching was very great. The area of phlogosis which had risen upwards on the mesocolon must have been equal to 60 or 80 square inches. Immediately after the bleedings the area contracted until it was less than 10 inches, having a solid resisting mass of phlegmonous nucleus, probably in the nature of typhlitis calculosa, the removal of which required many days. I believe that where you can cure a peritonitis by your venesection, and find some uncured remainders, it will be because they are phlegmonous remainders.

306. It is thus manifest that there should exist a much greater difficulty in procuring a resolution of inflammation, in uterine phlebitis, than in a peritonitis on account of the obstructions in

the efferent veins, as I explained in my sec. 242, as well as because there is phlegmonous disorder.

307. In whatever members of the childbed fever group, if the inflammation attains such an elevation as to transcend the power of cure by resolution, the crisis must end either in effusion, exudation, deposit, or gangrene; *i. e.* in effusion of serum and lymph, or *sero-pus*, or in the excretion of pure pus; or in *putrescentia uteri*, which is synonymous with ramollescence, or gangrene.

308. Let us suppose the case to be one of extensive peritonitis; one involving the intestinal tractus, the epiploon, mesentery, and mesocolon, as well as the entire intropelvic peritoneum. If it attains to the stage of effusion, you are to consider that effusion as the final act of the tragedy of the inflammation. In fact, the effusion is the issue or last term of the disorder, the inflammation ending with it. The turgescence ceases with it, the pain is over, the intolerance of pressure is gone, and the woman herself, as well as her friends, are rejoiced at the signs of amendment. She no longer dreads the least motion; she can turn, and even sit up in bed, and receives your salutation with the confidence that the danger and suffering are over. Let me advise you to read the touching account given by Alex. Gordon, of the case of Mrs. Low, the fifteenth in his table, to learn, how fatally her friends were misled by the subsidence of her pains, when they receive him "with transports of joy," while Gordon's own practised eye saw, in the stillness of the disease, only evidences of its mortal power.

309. The effusions, though they bring the active processes of the inflammation to a close, most generally, not always, lead to the speedy dissolution of the victim. It is not invariably the case that death follows effusions, so that all hope need not be lost as soon as percussion of the belly, and other means of diagnostication, convince us that the cavity has become filled with fluid.

310. It is quite possible the effusion may consist in very pure serum, and thus give rise to a real ascites, subject to a subsequent absorption and consequent cure. I saw a woman, in the old Philadelphia Almshouse, in Spruce Street, many years ago, whose belly was filled and greatly distended with effusions from an acute childbed peritonitis, which had assumed a hopeless charac-

ter. The case was pointed out and explained to me by the late Dr. Henry Neill, then one of the principal accoucheurs in Philadelphia. The patient had been long free from fever, had a good appetite, and seemed in all respects comfortable, except from the incommidity of so distended and heavy an abdomen. She had been thus far recovered for some 14 or 15 days; but as I afterwards learned, her health declined, and she died in the house. Other cases are noticed in which a spontaneous opening and discharge of the matters of effusion have taken place, with complete recovery of the women, one of whom subsequently bore children. Yet alas! when you shall hereafter meet childbed fever that has passed to the stage of effusion, you may conclude that nothing remains but to console and soothe the remaining few hours of the woman's existence.

311. Inquiries should be often made as to the state and progress of the lochial discharge. It is scarcely to be expected that the lochial excretion shall proceed, in cases of metritis or uterine phlebitis; and in discussing the differential diagnosis of peritonitis and the other forms, it will be always encouraging to learn that the lochia proceed favorably. If the woman suffers pain, referable to the womb itself; if its acts of involution be suspended; if the lochial matters cease to appear, or become abnormal in their hue, consistency, and odor, you will have greater reason to fear that the peritonitis is complicated with metritis or phlebitis of the womb.

312. Most assuredly you should come to the conclusion that this is the case whenever those singular intoxications, deliriums, and fits of insanity, or mania, are conjoined with pains, tympanitis, hurried pulsations and sinkings that are accompaniments of some of the peritoneal phlegmasias of childbed fevers.

313. For the present, I shall allow my pen to rest, with the purpose of continuing our communion in another letter, and I rest, &c.

LETTER XI.

ETIOLOGY—GERMAN NOTION OF ACUTE BLOOD-DISSOLUTION—KI-WISCH'S CASE—SIZY BLOOD OF PREGNANCY—FEBRICULA OF GRAVID WOMEN—CASE OF MRS. B. . . .—STATE OF THE UTERO-PLACENTAL DISK—UNDISCHARGED LOBULI OF THE PLACENTA—PRESSURE—CONTUSION—STATE OF CERVIX IN LABORS—OVER-DISTENSION OF WOMB—RELAXATION OF ABDOMINAL WALLS AND SUDDEN WITHDRAWAL OF TEXTURAL TENSION—TORPOR OF BOWELS IN THE GRAVID—EXALTED VITAL STATE IN LABORS, ANALOGOUS TO INTENSE HYSTERIA—ÆSTHESIS—DEBILITY FROM LABOR—COLD—UNREST—INDIGESTIONS—SEASONS; TENON'S TABLE OF THEM.

314. LET us to-day reason together concerning the causes of childbed fever; because if we can find ourselves well informed on that subject, we shall be able, not only to make use of sound prophylactic measures, but conduct our treatment more philosophically, in view of the real nature and force of the special cause in any case.

315. I have already, in former Letters, said so much relative to milk-metastasis, vitiation of the blood, epidemy, and contagion, that there is reason for me to be watchful, lest I should here reiterate parts of what I have already said; though, indeed, I do not suppose I should present a clear exposition of childbed fever in this Letter, unless I should venture on some iteration.

316. Women do certainly in some, but not in all instances, become disordered in health as pregnancy advances nearer and nearer to the term. They suffer from the weight, the volume, the distension, the pressure, and even the presence of the gravid womb. So vast a body as the uterus pregnant near term, could

not but affect the physical complacency of many among the 24,000,000 of women who are supposed to be pregnant throughout the whole world in each successive year. Yet it is by no means an uncommon thing to see them as merry and happy and as agile, as in their ordinary health. Indeed, I do suppose that for the immense majority of women who are gravid, their pregnancy does not at all make them fit to be regarded as sick women. Nor, indeed, could it be so, since the Divine Governor of the world would not design that the fulfilment of the most important office of reproduction should represent a diseased state. We should rather look upon pregnancy and labor, not as states of disease, but as the consummate expression of the highest physical power of the gentle sex.

317. On the other hand, women, not a few, are observed to suffer in gestation from torpid action of the alimentary canal; to have anorexia; to lose, in some degree, their wholesome sleep; to have swelling, or infiltration of the limbs; to complain of renal distresses; to have a frequent pulse and warm skin; a sense of fulness of the head; to have cardialgia; to become emaciated; to have a dry skin which is stained with ephelis; a bitter mouth, and a foul and yellow, or heated red tongue. Hydræmia is a frequent concomitant of pregnancy.

318. While, therefore, to be pregnant, does not in the majority imply to be sick, it does in certain instances mean that the health of the woman is disturbed. I need not remind you again of Willis's theory of ferments in the blood, nor of metastasis of milk, of which I discoursed so much at length in my third Letter; nor does it need, perhaps, that I should cite for you the opinion of Dr. Robt. Ferguson, in regard to a vitiation of the blood, which, in his judgment, "is demonstrably sufficient to produce all these phenomena," *i. e.* the phenomena of childbed fevers. Dr. Ferguson appears to have imbibed this notion from certain experiments made by the French physicians MM. Gaspard (*Journ. de Phys.*) and Cruveilhier (*Nouvelle Biblio. Med.*). Those experiments were made upon dogs, and consisted in injecting into their veins, and also into the areolar tissues, fetid pus (exp. 1), recent pus (exp. 5), and putrid pus (exp. 6). Likewise (exp. 14) half an ounce of *putrilage* made up of dog's blood and beef gravy; *putrilage* with

vinegar and water into the jugular vein; and lastly, ink and water into the femoral vein.

319. These were some of the matters injected into dogs' veins, to prove that, when a woman's peritoneum, womb, ovary, or uterine veins or lymphatics become inflamed, "the phenomena of puerperal fever originate in a vitiation of the fluids;" *op. cit.* 53. I presume that a very large majority of medical readers, perusing Dr. Ferguson's work, would be likely to swallow the above as so much excellent gospel; and deem it a settled point, because the French experimenters did such shocking things to those poor dogs (some of them, by the way, got over it), that a lady in childbed fever must have vitiated fluids as the cause of her illness or death. Don't you see, my dear young gentlemen, that though such horrid cruelty was done on the dumb beasts, it did not give them a childbed fever! and do you suppose that, if it could do so, you have any grounds to infer from a "putrilage of dog's blood and beef gravy," or ink and water in a dog's death, to a metro-peritoneo phlebitis in a woman! What a monstrous step in infer-ring! The *chaussure* for such a reasoning as this must surely consist in a pair of seven-league boots at least!

320. What some of the German authors call Blut-dissolution and Cholæmie, is a condition in which the blood has lost, in a measure, its vitality, and along with it its cohesiveness, or rather organization; and it may happen that, in certain contingencies of the health, we may meet with examples of cholæmia, where some of the principles of the bile, either through loss of secretory force, or through the resorptional operations, may exist in such excess in the blood as to disqualify it, to a certain extent, for its offices in the economy as either a nutrient or an oxygen-carrier.

321. Any person familiar with the phenomena and effects of phlebitis as observed in practice, ought, with readiness, to conclude that the vital state of extensive portions of veins may become altered in such a way as to give rise to a propensity to this blood-dissolution and cholæmia; and, assuredly, whoever has carefully studied the admirable essay of Mr. Dance, in vols. 18 and 19 of *Archives Générales*, must have no hesitation to connect the supposed state of the blood with a morbid condition of a vein or veins. Mr. Dance's essay is, *De la Phlébite Uterine, et de la Phle-*

bite en général, considérées principalement sous le rapport de leurs causes et de leurs complications. Every Student of Obstetricy who can procure that admirable essay, is not excusable if he fail to peruse it. Having preceded M. Tonnellé's paper, before alluded to, those two essays taken together are of inestimable value in the education of obstetric practitioners particularly.

322. *Die Geburtskunde*, &c. of Franz A. Kiwisch, II. Abtheil. 1 Heft, contains the remarks of that admirable writer on acute blood-dissolution and cholæmia of pregnant and lying-in women. He says it is allied to a diseased state of the blood in gestation, and that it sometimes exhibits its dangerous character in the clearest manner; and, as an example of the malady, he cites the following curious case, which is that of a woman, twenty-two years old, of a stout, well-developed frame, and who, on the afternoon of the 21st of January, 1851, came from the country to the Lying-in Hospital. She had hardly left the conveyance and mounted the first step when she fell, and with difficulty was got into the Labor Ward. It was her first pregnancy; slightly jaundiced; face cyanotic; extremities cold; pulse scarcely discerned; drops of dark thin blood from nostrils and genitalia; small ecchymoses on skin; could give no account of herself. The pains were feeble; os uteri large as a dollar, and tense; head high up; foetal heart plainly audible. At 10 P. M. she seemed to be sinking, and was then delivered with the forceps, in half an hour after which she expired. The dissection exhibited a tendency within the encephalon, thorax, and abdomen, to oozing of blood here and there from the tissues; but there were nowhere any positive marks of inflammation of the organs.

323. It appears to me that Kiwisch has here reported a case of what is commonly regarded as purpura hemorrhagica, under the title of *blut-dissolution*, and I have no objection to make to the word, which, indeed, is far preferable to purpura. I pray you note that this poor unfortunate, who reached the hospital but to die, must have been affected in such a way that the blood had lost its cohesive force or become disorganized, so that she no longer possessed the strength natural to one of her apparently well-developed and vigorous frame. She sunk down on the steps, and died in a few hours, with blood oozing from the nose, the genitalia, the

corpus mucosum cutis, the mucous membranes, and numerous points on the serous surfaces. What was this? If the power of the *membrana communis* had not been broken down, could her blood have become thus disorganized? Her vessels were filled with blood, which was already nearly dead when she arrived, because the blood-membrane was struck with a form of disease under which it could no longer continue its office of nervous induction into the blood it contained, and which it had created and hitherto ruled over.

324. How often have I, in my Class-room, urged you in your difficult cases to make the diagnosis by exclusions! There I have repeatedly instructed you as to the manner of inquiring into the state of the encephalon, and, finding the malady not to be there, to seek for it in the thorax, in the abdomen, in the articulations, the muscles, the skin, and in the organs of the senses. If the researches and interrogations addressed to the cavities, the organs, and functions, show the malady to be in none of these, then you have left only the blood to examine, for that is all that is left. But the blood's health and vitality is a direct dependency of the Endangium, and you can make out a blood disease, or endangial malady, with a certainty not at all less than that with which you conclude as to a tuberculosis or a pneumonia by the use of your percussions, stethoscopes, and other means of physical diagnosis.

325. I am well pleased to have recited for you Kiwisch's case of acute blood-dissolution, because it ought to show you that a pregnant woman may, even before labor begins, have her blood in such a condition as to destroy her suddenly, and also that the blood might become so greatly changed, under the irritations which, in pregnancy, supervene for some women, that there is the highest probability of diseases following the birth of the child.

326. I do not refer to such violent and fatal affections as that above related, but to those lesser derangements of the blood's erasis that leave the patient long imperfectly recovered from her accouchement; and in some instances, lead to topical embarrassment and partial stasis of the circulation, that end in establishing areas of phlegmasia.

327. When an area of phlogosis is once settled, it must ever be uncertain to what extent it shall expand, and what mischiefs to the health and life may ensue.

328. I have said in sect. 316, that to be pregnant does not, in the great majority, imply to be sick. It is, nevertheless, very true that you shall rarely observe that blood, drawn from a pregnant woman's arm, is without the evidences of a sizzly condition, or excessive evolution of its fibrin. In admitting this to be true, forget not that, as the Bible says, "they that are sick need a physician, not them that be well," and that you will not bleed nor medicate any persons who have no complaints requiring such a remedy. Therefore, when you see this sizzly blood, you are not necessarily to infer to the sizzly blood of all pregnant women, but only as to those who, being sick, require the remedy. There is good reason to suppose, however, that gravidity, with its last attending inconveniences, does often develop a fibrinous excess in the circulation, and that such excess comports with neither the health nor security of the woman, who, in labor, and during the consequent involutive processes, becomes extremely liable to inflammation of important tissues. In so far as this, I admit the blood to be occasionally vitiated in the gravid.

329. There are many women who, toward the close of gestation, become feverish; they are constipated; the urine is lessened, and is highly colored; the pulses are frequent; the breath is hot and accelerated; the mouth parched with a loaded tongue; they sleep unsoundly; the appetite is lost, and they complain of pains and aches, in the trunk as well as in the limbs. Such persons, when you come to make the diagnosis, do not exhibit any precise topical lesion as manifest causes of the constitutional disturbances, and we are compelled to attribute the evils complained of to the irritation of the nervous mass by the fact of the weight, the tension, and the intrusion of the gravidity. In these, the fluids cannot remain healthy, since some change in the crasis of the blood is an unavoidable dependency upon the vital state of the living solids immediately related to the hæmotosis. One is always apprehensive, therefore, as to the *post-partum* condition of such pregnant women.

330. I presume it is but an aggravation of this general *malaise*

that I have just mentioned, which takes the name of *Febris gravidarum et parturientium*, a most dangerous malady, that is apt, indeed, to show forth its whole strength and rage as soon as the acts of labor are accomplished.

331. Such a state as this I have observed in several examples during my long occupation as accoucheur, and I related the circumstances of one of them at the Philadelphia College of Physicians, in whose summary of their *Transactions* it was published. The illness in question was that of a young married lady in her fourth pregnancy, who had suffered from dropsy of the ovum, which had expanded the uterus so enormously, before the full time, that inflammation was in consequence developed, leading to her premature confinement, and resulting in her death a few days afterwards.

332. Many eminent authors, as Hippocrates, Burns, Jacquemier, and Kiwisch, have pointed to the dangers that environ women affected with fever at the end of the gestation. Kiwisch says it may occur in any of the months of pregnancy, but is more common at the seventh. That author, after describing the symptoms of the fever as commencing with rigors, and going on with headache, loss of appetite, dry tongue, inextinguishable thirst, &c. &c., gives three cases, one of which was "in the month of December, 1849. The fever began eight days before the birth of the child. The patient was brought into the hospital with a pulse of 140. The os uteri quite dry; violent thirst; altered expression of the face; yellow skin; faintness; the uterine contractions excessively painful. But, though the labor terminated naturally, the woman expired within six hours after its close, and exhibited, on dissection, the evidences of incipient puerperal fever, and blood-dissolution. You will, I presume, meet with such cases in your own practice, though rarely. Will you not be able to make out the diagnosis of such a malady, especially if you should resort to the method of diagnosing by exclusions? What will you make of the signs? Shall they be fevers? Shall they be mutations of the blood's crisis dependent upon affections of the solids of the vascular system? Shall they prove to be positive material phlegmasias—of the womb, of the veins of the womb, or of the peritoneum?

You are to be the sole judge in the matter, and upon your decision, probably will rest the life or death of the patient, since on your decision is hinged all your clinical treatment.

333. In every labor there is an act of the greatest importance to be performed by the woman—I mean the expulsion of the placenta. Look at Robert Lee's plate, and at Noortwyck's drawing of the gravid womb, to see how the orifices of the vessels look gaping into the womb's hollow, after the placenta is separated; or seize the first proper occasion to examine for yourself the placental superficies of a new-delivered uterus. You shall notice many openings, or foramina, of various sizes; some large enough to admit only a probe, and others that can take in a goose quill or a swan's quill. These openings lead into the hollow of many a large uterine vein and sinus. It is from them that the blood escapes after labor, in the shape of uterine hemorrhage, profuse or moderate, and also of what is called the common lochia.

334. The involution of the womb reduces these apertures perhaps to mere pores, or even closes them entirely; but many very discreet, wise persons of our Faculty insist that they are all and invariably affected with adhesive inflammation after labor, and that they could never recover but by that process. They aver that the disorder which we call milk-fever, by some German writers *wund-fieber*, is nothing but an adhesive phlebitis, arising as an indispensable process for the closing of these patulous orifices. I do not wish you to understand me as adhering to such an opinion as the above, but only as admitting that the exposed state of these apertures leading into the uterine veins does render the woman very liable to phlebitic attacks, and that, in all probability, many of the worst forms of uterine phlebitis do take their rise from these very points. A woman undergoing this adhesive phlebitis for the restoration of these parts, might well be supposed to be at risk, and that such risk consists in the conversion of the adhesive into a suppurative or pyogenic inflammation of the uterine veins and sinuses.

335. The fluids that accumulate in the cavity of the womb after the extrusion of the placenta, though not always, yet they do sometimes become highly offensive; to that degree, indeed, as to be promptly noticed upon entering a lying-in chamber. There are

some women, for whom no cares of cleanliness will ever suffice to quell this horrid odor of a *quasi* putrefaction. Indeed, there must be a sort of putrefactive decomposition of the blood and serum in the womb's cavity, into which air finds its way, in a temperature of 98 or 100° of Fahrenheit.

· 336. The lobules of the placenta do not always come off completely, one or more of them being not unfrequently left adhering to the surface; and even the most careful accoucheur cannot always succeed in clearing the organ of every vestige of the membranes and placenta. The deciduous coat of the womb is also now and then in an abnormal or hypertrophied state, which causes much of it to remain undischarged, though the practitioner imagines no traces to be left within. You are not to understand that on every occasion in which a lobule, or more than one, of the placental mass is left in the womb after labor, a serious illness will follow; for it is by no means a very rare event to find that even very large remnants of it are not expelled until the second or third days; and I have seen one case in which a portion weighing perhaps two ounces, was not discharged until the fifteenth day. You should know that a fragment of placenta left within the womb because its adherence was preternaturally strong, does not always at once die and become putrid, and so give rise to fever. If wholly detached, it would certainly putrefy—not otherwise; for Pouteau speaks truly when he says “L'enfant tient à la mère, comme l'arbre tient à la terre. Si l'ame vegetative a été anéantie par quelque cause dans les branches et le tronc, elle survit encore dans les racines.” Pouteau, *Œuvres Posthumes*, tom. iii. p. 14. Very well then, if large portions of placenta, and large plaques of chorion and amnion are left within the uterus, they not only undergo putrefaction themselves, but they entangle and detain within the organ portions of coagula and of mucus that are prone to rapid decomposition. Here there are fruitful causes of inflammation, and of pernicious influence upon the nervous system; causes sufficient to lay down the beginning of these childbed fevers we are studying. Such causes, acting upon a constitution rendered susceptible by pregnancy, by labor and its accidents, stand ready, like the hair-trigger of a rifle, to hurl the mischief at the slightest provocation, throughout all the parts within the pelvis and the abdomen.

337. In labors, the pressure of the child against the bladder and urethra, the soreness consequent upon the distensions and the obstructions of these parts, presently lead to what is called strangury, or retention of urine; and many a woman has such strange sensations after childbirth, that she cannot discriminate as to their particular nature and causes. Hence it is, that some of them allow the urine to collect within the bladder, until an enormous quantity puts the cyst on the stretch, almost to bursting. Many a time and oft have I abstracted with the catheter, forty or fifty ounces of concentrated urine; and I not long since measured sixty-five fluidounces that I drew off for a young woman, some days after the birth of the child. All such carelessness or ignorance is totally inexcusable in the monthly nurse and attendants, yet it is frequently to be deplored. Is there room to doubt of the dangerous tendency of such distensions as I have here mentioned? and is it not probable that the affluxions of an incipient area of phlogosis are sometimes determined and reinforced by these very states of cystic tension and painfulness?

338. A wise, prudent obstetrician will not, in labors, suffer the head of the foetus to rest too long upon the very same points, knowing that the arrest of the hard cranium, and its impingement, for hours together, upon the same points of resistance, cannot but excite a tendency to inflammatory reaction in the parts. He will always relieve it by raising the presenting part upwards in absence of the pains, or he will draw the head down with his forceps; or he will lessen the head and remove it, in order to save the woman's health, or life. When these cares are neglected, the areas of phlegmasia, or the foci of phlogosis are planted, and the expansion of them wakes up the whole train of a childbed fever.

339. The cervix uteri, in the virgin, is a cylinder; it becomes a cone in the pregnant women at term. The process of dilatation of the orifice, converts this cone into a wide open cylinder of four inches bore, and of a capacity great enough to transmit the foetus. In this process of dilatation, what a vast force of the pains is sometimes required, and what resistance! what distress, what anguish, what writhings, contortions, groans, and cries accompany it! The circle of the os is frequently torn and lacerated by the irresistible energy of the contractions, hastening to expel the

burden from the womb before the os can become dilatable. See then, in these circumstances, the sufficient foundations of phlegmasias, both membranous and phlegmonous, that readily pass into dangerous metritis, or metro-phlebitis of the womb. Under the tenesmic throes of labor, women bear down with enormous force, and thus compress the whole globe of the womb by their abdominal muscles and diaphragm. Coincidentally with this compression of the childbearing organ, everything within the belly is in like manner compressed, as if stamped by an iron heel, so to speak. Why then wonder, or why be astonished, to find the peritoneum in a flame of inflammation, and its areas of phlogosis spreading, like fire in a prairie, until everything that can burn, is burned with the fire of childbed fever inflammation?

340. The pregnant woman, at term, has the whole abdomen filled with the mass of the womb, which contains, perhaps a child of 10 pounds avoirdupois, a pound of water, a pound of placenta, its own weight being meanwhile from a pound and a half, to two pounds. I weighed two children, before I left the apartment in which they were born; one of them was 8 and the other $8\frac{1}{2}$ pounds in weight, besides the vast double placenta, and some three pints of liquor amnii; and I saw triplets, the sum of whose weight was 21 pounds. In Aug. 1854, I weighed twins of $8\frac{1}{4}$ each, and the placentas 2 pounds, and with probably 1 pound water—19 pounds in all. Neither of these women died, nor became very sick; yet it is well to call to your mind this distending effect of pregnancy, in order to suggest advertence to that excess of sudden relaxation that follows delivery. In practice, you will sometimes, in placing your hand on the abdomen, find the belly like a quagmire, so utterly without textural tension is it become after labor. I therefore wish you now to consider of the tendencies of the sanguine circulation within the abdominal cavity, brought about by this sudden and almost entire withdrawal of the pressure and resistance of the abdominal walls, and to inquire whether the whole tube of the aorta does not immediately become larger, by the lateral, or latitudinal out-thrust of the blood that is driven into it by every pulsation.

341. The conditions are such that this effect must ensue; and, in fact, a chief if not the only motive for the use of the binder,

that universally is applied for new-delivered women is, that it may safely counteract this tendency. Certainly, the failure to use the binder materially increases the probability of uterine hemorrhage and inflammatory turgor; and I have seen women, very faint for want of its support, who became comfortable soon after its adjustment. If it may please you, I should be well content that you refer to my *Letters to the Class*, third edition, p. 557, for the relation of a case that I explained upon the above-asserted principles. I have seen a good many of the same kind.

342. But I wish to call your attention to the effect of this suspension of the abdominal tension upon the rush of the streams that issue from the coeliac and the two mesenterics, which must be greatly augmented in quantity, and thus throw an excessive or hyperæmic circulation upon the vascular system of the entire alimentary apparatus, as well as upon all its serous membranes. I conceive it quite reasonable to infer, from this, to a general blush of those membranes, and an accidental stasis therein of masses of blood that become central areas, whence inflammation radiates or expands as puerperal peritonitis; and the thought suggests, that always, and most particularly in epidemic seasons, we should use the binder with reference to this its real prophylactic influence.

343. When the womb is gravid at term, the organ lies directly behind the front portion of the abdominal peritoneum, and in contact with it; no part of the bowels coming forward, or before the womb. In truth, the bowels are packed in a sort of triangular cavity, where their movements are much restrained and hindered, whence comes that tendency to torpor so generally complained of. For the most part, women near term do suffer from unknown and even unsuspected vast accumulations in the pouches, and in the canal of the colon; whence arises a heat and excitement, with fluxional determinations, that are additionally provoked and aggravated by the throes and struggles of the labor. I therefore conclude that constipation is a fruitful source of those vital conditions within the belly that end in establishing the foci of inflammation. *Vide* sect. 546.

344. Few women pass through the stages of labor without augmentation in the number, volume, and force of those jets that are

driven into the arteries by every systole of the heart. True, we observe some of the sex who never exalt the circulation, in the least, during the acts of parturition. Such, however, are cases of exception, not of rule. My own experience teaches me that in an active, energetic, and healthy labor, the pulse ought to become greater in volume, force, and frequency; and that this exaltation should coincide with higher respiration and a fuller evolution of animal heat. This gives a degree of excitement very analogous to fever excitement.

345. Floods of perspiration streaming from every pore on the head, neck, and thorax, keep down this paroxysmal state within safe limits; yet it is true to say, that the exaggerated rate of the blood's flowing does, often, require many hours to allow of its subsidence after the labor is over. I call upon you to reflect upon the possible modifications of the blood itself, introduced by such a *quasi* febrile paroxysm, and the eventual stasis of portions of the intro-abdominal circulation, where areas of phlogosis may thus be established.

346. The panic fear, the astonishment, the anguish of the labor

“*Hæc volnera vitæ*

Non minimaum partem mortis formidine aluntur.”

LUCRETIVS, III. 63.

—are they of no influence to disorder and derange the protecting, governing, providential power of the nervous mass over its subject organs? It is well, in studying this subject, to bear in mind the state of the woman's intelligent soul; that you may obviate, by kind assurances, by reasonable expectations pointed out, by clear exploration of her state and prospects, and by some “sweet oblivious antidote,” the mischievous tendency of unfounded alarms and confounding terrors.

347. I went to a hotel to see a young lady, who, upon arriving from a distant State, was suddenly seized with the unlooked-for pains of her labor. She was much nearer to term than she had imagined, and being in a land of strangers, accompanied only by a young husband, you may well conceive of her fright under the circumstances. One of the hotel servant-girls and the husband were the only assistants. She was far from her father and mother and her safe home. She was thoroughly panic-struck, and with

every pain writhed like a wounded worm, uttering groans and cries of anguish and affright. Seeing that her moral state was a perilous one, I resolved to change it, and I said, "Look at me, my dear! look steadily at me, and hear what I have to say." She fixed her eyes upon my countenance for a short time, when I added: "Do you know, my daughter, that you are behaving like a little fool? Do you know that if you are frightened for nothing you will be really sick by and by; whereas, now, you are only in labor, and not sick at all. Listen! when your next pain comes, say to me in a soft and gentle voice, 'Doctor, I feel the pain coming on!' Will you do so? 'Yes, sir!' Very well, then, I will show you how to bear it." In a little while she said very calmly: "The pain's coming." "Very well, then; now, say not a word. I shall press your shoulder very hard. There, now, the pain is half over; now, it is three-quarters gone; and now—does it hurt you still?" "No sir, not very much." From this time out, until the child was born, she never uttered a complaint, and, indeed, went through her primipara trial with the most admirable patience, and gentleness, and decency. Can you guide your patients so? It will be well for many of them if you can; for if you can quell the panic she can bear the pain, and will add patience to the faith you have inspired.

348. Women, after their confinement, are often ill-guided by the monthly nurses, who give improper food or drinks. Indigestible food, or too much food of any kind, lays the foundation of inflammation by the load, the acidity, the saburra thus introduced.

349. Imprudent exposure to cold and damp blasts; the drinking copious draughts of iced water; the eating of iced creams; too early rising in bed; some ill news; some shock to the feelings; all these have served as the foundations of a childbed fever inflammation.

350. The seasons of the year may serve as causes of childbed fevers. Thus you will find in *Mémoires sur les Hôpitaux de Paris*, by M. Tenon, Paris, quarto, 1788, at p. 261, a series of tables, giving statistics of the number of children and of lying-in women dying in the Hôtel Dieu, in the several months of each year from 1776 to 1786 inclusive. From Tenon's tables I have

made out the following to show how many children were born in each month of those ten consecutive years, and the number of lying-in women who died in the same period:—

351. Table of births, with statement of the number of women who died in each month, at the Hôtel Dieu, for ten years, from 1776 to 1786 inclusive—

	Births.	Deaths.
In January	1604	127
In February	1547	130
In March	1623	101
In April	1460	104
May	1438	91
June	1228	54
July	1237	58
August	1238	57
September	1224	57
October	1294	73
November	1242	137
December	1391	169
Total in ten years . .	16526	1158

352. This table shows that while the mortality at Hôtel Dieu was so horrible, the least fatal months were June, July, August, September, and October, while December was the most fatal of the months for a period of ten years. Yet it should be observed that, December, 1779, gave only four deaths, five in 1784, and two in 1785; May gave thirty-three deaths in 1778.

353. To infer from the above table, as well as from one's own private experience in practice, childbed fevers are less frequently to be dreaded in the mild than in the variable and cold seasons of the year. Certainly, this may be stated with truth of the population of Philadelphia.

354. As soon as labor is ended, the process of womb-involution is fairly commenced. The evolution required a series of 280 days to make it complete; the acts of involution by which the organ regains its non-gravid volume and quality, requires at least thirty if not forty days. Now the utmost necessity exists for the regular performance of these acts of involution; any arrest, sus-

pension, or even considerable retardation of which is at once a disease. The evolution of the gravid womb took place as a normal physiological hypertrophy, whose limits are specifically assigned. During the process of return, or involution, this hypertrophy is continually being recovered from, and while the reduction goes regularly on, it is normal hypertrophy; but its suspension or arrest, immediately converts the lying-in uterus into a pathologically hypertrophied organ, a diseased organ. I think that you will see in the above, reasons why childbed fevers should be so destructive, not merely because they are inflammations, but because they interfere with one of the most important acts of the economy of the woman. Having, perhaps, exhausted your attention by these long details, I shall now close this communication, intending to pursue another branch in the history of the causation in my next.

LETTER XII.

HUMBOLDT ON CLIMATE—EPIDEMY—LA ROCHE ON MIASM—COSMIC AGENTS—FILTH—TENON'S STATE OF THE SICK AT HÔTEL DIEU—INFLUENCE OF THESE ON THE NERVOUS MASS—VIEWS OF OKEN, CUVIER, AND FLOURENS, ON THE NERVOUS MASS—THE ANIMAL NAUGHT BUT NERVE—MODIFICATIONS OF NERVE CRISIS CAUSED BY EPIDEMY—M. CERISE—WEAKNESS OF A CAUSE THAT AFFECTS ONLY ONE CLASS OUT OF MANY—ACCIDENTS.

355. "THE expression climate, taken in its most general acceptance, serves to designate the ensemble of those atmospheric variations, which in a sensible manner affect our organs; the temperature, the humidity, the changes in barometric pressure, the calm of the air, winds, the greater or less tension of the atmospheric electricity, the purity of the air, or the presence of miasms, more or less deleterious, and, in fine, the ordinary degree of transparency and serenity of the sky. The last condition influences not only the results of the calorific radiation of the earth, the organic development of plants, and the ripening of fruits, but also the moral state of man, and the harmony of his faculties." —*Cosmos* I. 377.

356. I have extracted the foregoing paragraph from Baron Humboldt's *Cosmos*, because, having a bearing on the evolution of the disorders we are here studying, I think that you ought to take into consideration not only the meaning of the word climate, but also the influence of it on the pregnant and parturient portion of our people. Baron Humboldt's examination of the influence of climate, following the excerpt above, would be highly interesting to you as a study, which, I advise you to seize the first opportunity to enjoy. Our common expressions in regard to the

weather, as fine, bad, favorable, wholesome, unwholesome, bracing, relaxing, exhausting, &c. &c., show that the people at large, do appreciate its influence upon our vital state. Not only are we affected by the thermometrical condition of the air, but by its moisture and dryness, its pressure and electricity. We live, indeed, here upon the earth's surface, at the bottom of an atmospheric ocean, as Humboldt calls it, and we cannot but be influenced by the state of the medium in which we live, and move, and have our being. Hence, I say, that climate, or the conditions included in the meaning of the word climate, may so affect pregnant and lying-in women, as to give, on the one hand, a great tendency to fall into childbed fevers, or on the other, to hold them in a great degree exempt from them. There is, it is true, much difference of opinion to be found in our medical authorities, in regard to the influence of particular meteorological states on the bringing about of frequent childbed fever cases. It is by no means a settled proposition what sort of weather is most favorable to its rise among the population. I may refer you to my table from Tenon in section (351), and if you desire to follow a very learned discussion on the subject, I may also point you to that very singular book, *Die Kindbettfieber, ein naturhistorischer Versuch*, von Dr. Eisenmann, Erlangen, 8vo. 1834. The author is of Munich, in Bavaria. He treats the question sec. 60, and page 150. I may not use space to make excerpts from his interesting volume.

357. It is by no means just to inculcate the idea that epidemics are dependencies upon the weather merely, yet it is true that we meet among the population, occasionally, very numerous cases of some particular forms of disorder that are suspected of being epidemical; whereas, they are not epidemic, but only weatherly or climatic affections, beginning and ending with certain states of the weather. You may hereafter find, therefore, in your practice, that many women shall fall sick with childbed fevers, and in such numbers as to lead you to say that the disease is epidemic, when, in fact, it is not epidemic, but only sporadic, or let me say accidental, weatherly and accidental.

358. As to epidemic cause, I know not what it is. I know not what miasm is. Pray, open Dr. Dunglison's *Med. Dictionary*, at the

word Miasma, and see for yourselves what that learned author has set down for our information. I beg also to refer you to that most learned and admirable volume of our townsman, Dr. R. La Roche, on *Pneumonia ; its supposed Connection, Pathological and Etiological, with Autumnal Fevers ; including an Inquiry into the Existence and Morbid Agency of Malaria*. At page 116, he discusses the question that the non-detection of malaria is no proof of its non-existence and agency : "It has been urged time after time," says Dr. La Roche, "in opposition to the malarial origin of fever, that the existence of the effluvia, to the agency of which the disease is ascribed, has never been proved ; that their presence in the atmosphere has been inferred from the effects observed rather than positively demonstrated ; that they have so far eluded detection ; and that the air of sickly localities, whenever subjected to chemical analysis, has been found to contain the same ingredients, and in nearly the same proportions, as that of salubrious places. Much of this is doubtless true, and will not be denied by any one who has endeavored to make himself acquainted with the state of knowledge on this subject. All are aware, for the fact has been often referred to, that the experiments of Gattoni, and others, led to the conclusion that the air of the marshes of Fuentes is as pure as that of Mount Legone, one of the Grison Alps, at an elevation of 8,040 feet above the level of the sea. They are aware that, at Martinique, Moreau de Jonnes found no difference between the chemical composition of the air of Mount Tartanson, at a height of some six hundred feet above the level of the sea, and the valley of Case Navire ; and that the atmosphere of the wards of the hospital at Fort Royal, during the prevalence of yellow fever, differed from neither. They know that Julia de Fontenelle could detect no deleterious gases or foreign chemical ingredients in the atmosphere of infected localities ; that the latter did not vary from that of healthy places in any of the principles which chemical analysis enables us to discover ; a result he obtained in the marshes of Cerde, near Narbonne, at the Pond of Pudre, at Salces, Salanque, Capeatang, Cette, at Barcelona during the fever of 1821, and at Paris during the cholera of 1833. They also know that a distinguished chemist and professor in the school of Montpellier, Bérard, reached the same conclusions from his experiments on the pestiferous air of the marshes of the coast

of Cette; and that Désage obtained the same elements in the most confined marshes as on the most exposed hills." The careful study of Dr. La Roche's work will prove to your satisfaction, not only the existence of malaria, but convince you that mankind are able to obviate its force, and even abolish its source on many occasions, by sanitary measures. I am sure, you could desire no stronger proofs of the power to abolish the very sources of the foulest *aria cattiva*, or *mal-aria*, than that I have brought forward in sects. 133-7, relating Prof. von Busch's success in disinfecting his lying-in hospital by heat, or Dr. Collins's disinfecting methods at Dublin Hospital.

359. I have long since said that the causes of Epidemic disorders may, with probability, be assigned to certain relations that exist between the deep-seated structures of the earth and the atmospheric ocean at the bottom of which we live. Nor is it at all an objection to such an hypothesis that the chemists have been unable, hitherto, to detect, as you see, by my excerpt from La Roche, any substances, gaseous or other, in the atmosphere, that might serve to render it deleterious. See also the admirable work of Dr. Monfalcon, *Sur les Marais*, for the results of many eudiometrical and chemical examinations of the atmosphere in celebrated malarious places, for many confirmations of Dr. La Roche's statements.

The catalogue of simple substances and of gases, whether simple or compound, is far from being as yet complete; and it is no wild stretch of the imagination to suppose that what Sydenham calls constitutions of the air, may derive their morbid characteristics from the presence of deleterious substances that rise from beneath the lowest strata of our globe to render the atmosphere poisonous over vast regions of the earth's surface. Certainly, those modifications of the atmosphere, which introduce epidemical constitutions of it, have no known relation to its heat, its humidity, its pressure, its electric state, or its chemical composition. Yet they do exist; and poison not without a cause. They not only exist, but are capable of concentration in certain places; as in jails, hospitals, and ships, where they exert with peculiar intensity, their destructive powers; while, at the same time, malaria undeniably exists in the calmest plain, on the

steepest hill-side, or out on the open sea; as if a cloud, invisible, should rest above a city, a nation, a continent, or a sea-tract, containing within it the elements of death, fatal as the sword of the angel above the sleeping hosts of the Assyrians of old.

360. There are then atmospheric causes, that render pregnant and lying-in women, at particular times and in certain places, uncommonly liable to attacks of childbed-fever-inflammation. What the real principle of this Epidemy is, I believe there is no man can say. Be it what it may, one of the most extraordinary conditions connected with it is this: that it should not poison men, nor boys, or girls, or non-pregnant women, but only the pregnant or lying-in portions of society. This appears to me to be its greatest mystery; and this, I think, it is, that has led to the adoption of the erroneous and mischievous idea of its communicability, transmissibility, contagiousness, or infectiousness. Choose you the expression most befitting.

361. It is common to attribute epidemic disorders to circumstances connected with filthiness or impurity. But who can impute filthiness and uncleanness to a charming *petite maitresse*, who is more scrupulous about dirt than about her religion, and acts under the dogma that cleanliness is next to Godliness. To be sure, when childbed fevers are epidemic, you would be more apt to fear the attack for women crowded together in ill-ventilated hospital-wards, and packed, four in a bed, as at the Hôtel Dieu; and this fear is well founded; but at the same time, in epidemic seasons, you are almost as anxious in regard to patients in your ten-thousand-a-year-dom, as for your poor; and it is unhappily too true, that in the general and wide-spreading comprehensiveness of epidemic childbed-fever, those women, who, from education, habits, and modes of life, are become most highly sensitive and æsthetic, are among the first victims, owing perhaps to the exceeding impressionability connected with their social position and rank. Do not then bring your accusations of uncleanness here, but rather look into the epidemic constitution of the air, if haply you may descry some oozone, some ova of animalcula, some spores of vegetables floating in the breeze, and entering, like a thief in the night, into the fold of the body, to rob it of its treasured soul.

362. Lying-in hospitals, with rare exceptions, have proved rather a curse than a blessing to the poor. Hear what Tenon, *op. citat.* p. 238, says of the celebrated Hôtel Dieu, at Paris, in 1788.

363. "The situation of the accouchées at the Hôtel Dieu is most deplorable; they are often placed two, three, and sometimes four in a bed, some in their lying-in, and some at their other epochs; their natural evacuations infect them the more as these beds become heated by this state of pressure, the health of the women giving way, as the humors grow more corrupt. The torments they endure are aggravated by the events that follow the labor, such as tension and pain in the breasts, head, and belly; milk-fever, and an acid sweat that supervenes. Is it not in those beds, where the well are confounded with the sick accouchée, and with those seized with puerperal fever, that the cause of so many deaths exists? What is the health that could hold out under such frightful circumstances? What disease might not arise? Open one of these beds of misery, and there escapes from it, as from the mouth of a cavern, a damp hot cloud, which floats, spreads about, thickens the air, and gives it a body so manifest, that in a winter morning it opens to let one pass through it, although that cannot be done without an insurmountable feeling of disgust."

364. After looking then at Tenon's picture, are you surprised at the table I made for you in sect. 351; or do you wonder how it could be that Tonnellé had the opportunity to dissect two hundred and twenty-two women dead of childbed fever in a Paris hospital? *Vide* sec. 226.

365. But no matter what it is that gives a propensity to the attack of childbed fever in its epidemic form; whether it be season, climate, atmosphere, or rather epidemic constitution—malaria, idio-miasm, or koino-miasm: what we have to learn is this; in what way or manner does this influence, poison, or nebula affect us? What part of us does it attack, on what does it impinge, and how doth it afterwards effect its morbid modifications? These are the questions for us to solve; if we could but do so great a thing!

366. Professor Oken, Baron Cuvier, MM. Flourens and Unzer

have ascribed to the nervous material of living bodies, not only the sensational faculty, but that of originating all vital motion; and if this teaching be just and true, then Oken is correct in saying that an animal is naught but nerve—a sentiment fully sustained by M. Flourens, in his admirable *Résumé of the Labors of Georges Cuvier*.

367. For my own part, I long ago yielded up my faith, upon a conviction that the physio-philosophers are right in regarding the animal as naught but nerve, or as productions and evolutions from nervous mass; and I must beg you to have patience with me, while I here reiterate the opinions I have elsewhere advocated on the subject; for I can by no means expose to you my whole thoughts on childbed fever, unless I be allowed first to explain my opinions upon the nervous system.

368. No impression can be made on any living body unless it be upon the sentient extremity of a nerve, whence the impression proceeds, in order to its reference to the brain or to the spinal cord or to some ganglion, or some system of nerves within an organ, or a tissue. Every impression must awaken a reflex force, whose operation determines a motion in some part of the animal machine.

369. In breathing hot-blooded animals, the oxygen they require is transmitted into the blood flowing in the capillary branches of the pulmonary artery, and the blood is therefore the common oxygen-carrier for the whole economy. The blood, then, is an oxygeniferous body, and there is no point or tissue to which the blood does not carry this oxygen, without whose presence there is and can be no life. One of the conditions of the problem of life, is the presence and agency of oxygen. Wherever it moves within the economy, it makes impressions on sentient extremities, or on nervous mass, and motion is determined as the next effect. But the living blood may carry, not oxygen only, but chloroform, ether, and various odoriferous substances and medicinal things, which each makes its specific, peculiar impression within, and awakens a resultant peculiar motion. If now, the atmosphere be replete with the physical cause or poison of cholera, of yellow fever, or ship fever, &c. &c., why should it not, in like manner, be replete with a material that shall so modify action as resultant of impression, as to make hundreds of women

prone to childbed-fever-inflammations, and yet not be of force to awaken diseased action in any other class of persons in all society?

370. In the human being, and in all the higher animals, the animal machine consists in its nervous mass, and the organs and tissues that have been first created and evolved by it, and through its power afterwards fully developed and sustained, and controlled by its force.

371. Thus, the eye is but the tubercula quadrigemina, that has protruded itself into the bony orbit, and there fashioned its lens, its iris, its cornea, and all the apparatus by which it is enabled to receive the impressions of light. So of the ear, and the organ of smelling and tasting. The arm and hand is a prehensile organ, that the nervous mass protruded from the bud-like germ that existed on the early embryo, and which, itself, budded out from the spinal cord. The same is true with regard to the locomotive members, the legs, which are dependencies and productions from the cerebro-spinal and sympathetic nervous masses.

372. A lung is a breathing or oxygenating nerve, immensely subdivided, expanded and clothed with the machinery indispensable for the exercise of the respiratory acts. The liver, the spleen, and pancreas, the kidneys, and the alimentary canal, the skin—in fine, all the organs and tissues, are but the agents, servants, and absolute dependents for existence, sustenance, and power, upon the regent and dominant nervous and creating nervous mass—the sole perceptive and sole motive-power in an animal economy.

373. Everything within the circumference of a body dies, except it be kept alive by the continual operation of the nervous force, proceeding from the nervous mass.

The blood itself, dies immediately, upon the dissolution of the connection between it and the living solid of the endangium on which it depends, and which is itself a dependency, equally absolute, upon the nervous mass that created, evolved, and sustains it.

374. I am very sure, my young friends, that what I have above said will excite a smile in many a reader, and possibly many of you who have so often heard me express these opinions

in our class-room, will prove equally incredulous as to my exposition. If the limits of this volume could permit, I should here endeavor to sustain my views by many an excerpt from M. Cerise's paper on nervous sur-excitement, in the *Trans. of the Roy. Acad. of Medicine* at Paris. But I am obliged to forego the satisfaction of showing you, from that author, how reasonable is the thought that every emanation of the nervous power from the centres, is the result of contact of arterial or oxygen-blood with nervous matter. But I will here cite M. Cerise's words from a part of his analytical *résumé* and conclusions, vol. ix. p. 290.

"All nervous excitation is, therefore, the result of the concurrence of a sanguine with a nervous element. All nervous excitement, therefore, may be represented by a product formed by the contact of these two elements.

"This product, considered in its elements, may be denominated neuro-arterial product, or neurosity. Neurosity is a force, which, being disengaged in a peripheral excitation, is transmitted to a determinate point of the nervous centre, and reciprocally; in the former case, it is an act of impression; in the latter, of innervation."

375. But I must not yield to the temptation which would seduce me to more copious excerpts from that admirable exegesis; and perhaps what I have premised may suffice to show you that I suppose certain conditions of the nervous mass of individuals, or even of vast numbers of a population, may be brought about by epidemic forces, that allow the subject organs to fall into certain specific modes of disease, which disease will depend upon the original and peculiar impression made by the morbid cause; *e. g.*, marsh miasm shall make impressions resulting in intermittent, remittent, and continued bilious fever. Other miasms shall develop the innervations peculiar to yellow fever, to Syrian plague, sweating sickness, miliary and petechial fever, cholera asphyxia, jail and ship fevers, childbed fevers, and so through the whole catalogue of idio and koino-miasms, or zymotic disorders.

376. Now what difficulty, what hesitation, indeed, should we have to admit that the nervous mass, being placed by the force of

epidemical impressions in a certain condition, and imbued with certain dispositions and propensities, the circumstances attending pregnancy and labor, should admit of or provoke the explosion of the otherwise inappreciable force of those qualitative states of the nervous mass, that unquestionably can be established by such poisons, venoms, malarias, and epidemics?

377. Whatever may be the nature of those agencies that render childbed fevers epidemic, it is clear they must be feeble as to human beings in general. Since they can exhibit their force only under the favoring and inviting conditions, in which pregnancy, labor, and lying-in, place our women.

378. Is it a peculiar principle or force? Look over the table of epidemics at section 197; remember the one presented at section 172; recall the 222 dissections by Tonnellé, or open your eyes to the too obvious clinical manifestations that occasionally shock every much employed accoucheur.

379. If the above exposition carries with it even the resemblance of reasonableness, then where shall a man stand that he may preach the doctrine of the contagiousness of childbed fevers, a doctrine which I am profoundly convinced could not dwell in the spirit of any person who is truly well educated in Obstetrics, whether as a science or as an art!

380. I shall say nothing of wounds and other accidents, which, in women pregnant, or recently delivered, might bring about those terrible intro-pelvic and abdominal inflammations, that constitute the essence of childbed fever, and which are indeed the *ipsissimus morbus* in all the cases; not because I fully concur in the opinion of Kiwisch, that such as these ought to be excluded, but that I may not unnecessarily extend these pages. Nor shall I spend my time and yours by further essaying to prove that there is no such disease as childbed fever, but only grave inflammations of the womb—its veins, its ovaria, its ligaments, its peritoneum, or the general serous lining of the interior of the belly. It will be my duty to show that in the treatment, you will have to address your therapeutical power to the subduction of these inflammations, and that the measures which are inapplicable to this

end, are futile for all other ends in the cases. In short, I shall endeavor to inculcate the opinions and practice of Alexander Gordon, who was truly our reformer and guide in this especial part of practice, and whose memory should on that account be cherished by every Scholar of the Class of Medical men throughout the whole wide world.

C. D. M.

LETTER XIII.

COLLINS CITED TO SHOW THAT TYPHUS FEVER IS CONFOUNDED WITH CHILDBED FEVER—PREGNANT WOMEN IN CHOLERA—KIWISCH'S TESTIMONY—TYPHOID FEVER NOT CONNECTED WITH CHILDBED STATES—SMALLPOX AND CHILDBED FEVER—THE WOMB COMPARED TO A STUMP IN AMPUTATIONS—REMARKS ON THE CONTAGION OF TYPHUS AND OTHER ZYMOTIC DISEASES.

381. IN attempting to lay before you my views upon the etiology of childbed fevers, I have restrained my pen within what I deemed the proper bounds of those disorders; but I have nowhere stated that all women, perishing during the lying-in, must be held to die of childbed fevers.

382. The volume now lying before me is a very celebrated one; celebrated for the copiousness, accuracy, and conscientious severity of its relations. It is a *Practical Treatise on Midwifery, containing the result of sixteen thousand six hundred and fifty-four Births, occurring in the Dublin Lying-in Hospital, during a period of seven years; commencing November, 1826.* By Robert Collins, M. D., late Master of the Institution. London, 8vo. 1836. This celebrated volume has added a world-wide fame and honor to the name of its excellent author, because, not only is it richly stored with valuable facts, but when one looks for the truth among its pages, he may not fear to find a falsehood.

383. Dr. Collins was Master of the Dublin Lying-in Hospital for seven consecutive years; and what he saw and did there is put down on these honest pages of his. I should be very glad to believe that every Student of my Class will read it with care, for it is too large and too full for me to bring forward all even of what he says on the "Puerperal fever." Let me, however, quote

from 380. "In private practice among the higher classes in Dublin, puerperal fever, accompanied by the *low typhoid symptoms* so prevalent in hospitals, is scarcely known." He adds, that Dr. Joseph Clark, in forty-five years, had lost only "*four patients from this disease*," by which, we are to understand, this low typhoid form. Further; it does prevail among the "lower classes in Dublin," and of "the same character as is observed in hospitals," but never to any extent.

384. I believe that typhus is frequently met with among the very poor classes, not in Dublin only, but in many parts of Ireland, as well as in other countries; and I have no objection to offer, if you should say, that, during the reign of epidemic typhus, women in hospitals and in ill-ventilated private houses, ought to be considered very fit subjects for the action of typhus malaria, during their pregnant and lying-in states. Dr. Collins, at 381, mentions two instances of women brought in sick with typhus fever, and placed in wards with other women, not sick at the time. Some of these women, however, sickened and died; though in another case, when a typhus patient was admitted, and died on the second day, no sickness ensued among the women of the ward.

385. Dr. Collins gives us very plain relations of the cases of many of his patients who recovered, as well as of many who died. I am not sure that all who died, in this class, ought to be ranged among the victims of childbed fever; for there is no more reason why a jail fever or a typhus fever patient, should not perish in her lying-in, than there is why a pregnant, or new-delivered woman should not perish with epidemic cholera or epidemic smallpox. Dr. Collins's patient, No. 95 (*vid.* p. 447), "aged 36, was delivered Feb. 19, at 6 A. M., of her 8th child, after a labor of one hour." At 11½ P. M. same day, slight pain and tenderness of belly. Had had 4 grs. calomel, and 8 of jalap some hours before delivery; it had not operated, and she now got enema with turpentine, and also stuping. Pain rapidly augmenting, became in half an hour "extremely acute." Enema acted well. Four dozen leeches; stupes. 20th, 6 A. M., pulse 120, feeble; 6 drachms castor-oil, and 6 dr. turpentine were given, and other ordinances were made which need not be here cited; but, at 9 A. M., pulse 120, not to be felt

in left arm; belly soft, but full; countenance sunk; bowels free. 9 P. M., pulse 130, scarcely distinguishable! 21st, 10 A. M., pulseless; belly soft and little painful on pressure. Died a few minutes past 11 A. M., after an illness of thirty-six hours.

386. There is Dr. Collins's case for your consideration. What do you find to say of it? Was this one of our childbed fever cases, or no? "On dissection, not more than two or three ounces of bloody serum were found in the abdominal cavity, without any lymph: The serous membrane exhibited very little, if any vascularity; chest healthy; no other morbid appearances. She was a poor starved creature." P. 449.

387. And now that you have learned the *post-mortem* revelations, what say you? Was this a childbed fever, or no? You can decide for yourself; as for me, I admit that she died in childbed; but if she died of childbed fever, then this doctrine of mine is wholly wrong, in that it denies the very existence of a childbed fever.

388. Kiwisch had 15 cases of cholera, in pregnant and lying-in women, in his hospital, during the epidemic there. Some of them recovered.

I saw a young woman in advanced cholera, confined at seven months, in 1832. She recovered happily, but if the psorenteric stage of the cholera had been reached, she would, probably, have perished with supervening childbed fever. Many is the case of sporadic childbed fever, that issues forth from what was originally merely a "weed," or irritation of the mammary gland; and why should not a milk-fever convert itself into a metro-peritonitic or phlebitic fever, or why should not an accidental rigor, resulting from a slight exposure of the person to cold damp, or following some moral shock, end in laying the foundation of those areas of phlogosis, which, once established, tend rapidly to expand, and afterwards prove the dominants in those metamorphoses of the tissues and excretes, that lead to dissolution? Within the past twelve months, I witnessed the dissolution of a young lady, a primipara, who perished with metro-peritonitis developed near the third week of her confinement, by means of a most painful, extensive mammary abscess, that, awakening the train of an inflammatory fever, *fell*, to use a common parlance, upon the womb and peritoneum with mortal violence.

389. Not many examples are met with of what is called typhoid fever, among our class of patients; and, when they do occur, they have been known to pass through all the phases of abdominal typhus, until the close, without developing our peculiar phlegmasia.

390. In like manner, women in labor and confinement, attacked with variola or scarlatina, though exposed by the circumstances to extreme peril, do, as I have witnessed, pass unharmed through those diseases. Yet it is generally to be deemed a fatal thing to be assaulted by either of them.

391. Though I am not prepared, with certain writers, to compare the new-delivered womb to a stump after an amputation; I am, nevertheless, convinced that the condition of the organ, especially as to its placental superficies, is one extremely critical, owing to the compact nature of the human afterbirth, all whose lobuli are united into a single cake-like disk. The pachyderms, the ruminants, the carnivora, and other creatures in whom the placenta, which is an excrescence from the exterior of the chorion, is more universally distributed over the inner face of the uterus, escape many dangers and accidents to which our women are exposed. Those accidents seem to have an almost indefeasible tendency to light up the fires of inflammation, in the child-bearing tissues, after delivery; and the fact, undeniably, is, that no accoucheur who thoroughly understands his mission as a professional man, is ever completely exempt from a feeling of uncertainty and anxiety as to the safety of his patients in childbed, until time sufficient has elapsed to permit the involutive processes of the womb to make a certain progress towards the non-gravid condition.

392. I beg you now to understand me as asserting, that while childbed fever is a phlegmasia, and that while there is not, in our nosology, such a thing as a true idiopathic childbed fever, I yet admit the possibility of typhus, jail, hospital, and ship fevers occurring in our class of patients. Some of the cases proceeding to their solution in recovery or death, without interesting, in a particular manner, the childbearing organs, or the peritoneum; while there are others, that early establish areas of phlogosis which may or may not take up the mastery in the subsequent progress of the malady.

393. But all such cases as these are fevers, jail, ship, or hospital; and the childbed-fever-inflammations are occurrences after the fact, and not before it. Let us endeavor to keep our minds clear and free from the shackles of generalization.

394. If typhus is contagious, then in typhus among pregnant women, there may be a power of transmissibility by contact or *ad distans*, and a convertibility into childbed fever complications. But I never have believed that typhus is contagious, nor never can, though I can readily comprehend why a person should be seized with it, on going into the malaria which causes it. While I deny the contagion of cholera and yellow fever, I should prefer not to inspire the choleric *El Hawa* of the Arabs, and would gladly avoid the local sources of the latter. Even so, with my patients in childbed fever inflammations, persons to whom I should be very loth to communicate the inoculable virulency of a dissection wound; not purely because erysipelas and metritis are contagious disorders, but purely because a newly-delivered woman, like a bundle of flax, or like a pistol-pan of powder, is likely to be inflamed and go off for the smallest spark.

C. D. M.

LETTER XIV.

ERYSIPELAS AND PERITONITIS COMPARED—ERROR TO CONSIDER
CHILDBED FEVER AN ERYSIPELAS, SINCE IT IS A GROUP OF BOTH
PHLEGMONOUS AND MEMBRANOUS INFLAMMATIONS—DR. PHILIP
PITT WALSH'S VIEWS ON PUERPERAL FEVER—DR. J. Y. SIMPSON,
OF EDINBURGH, ON IDENTITY OF ERYSIPELAS AND PHLEBITIS—
ONTOLOGISTS.

395. You have long since become acquainted with the fact that some relation, or perhaps, identity is believed to exist between childbed fever and erysipelas, and that, during the reign of an epidemic erysipelas, particularly in general hospitals connected with lying-in hospitals, much anxiety is usually felt for the safety of pregnant and puerperal women. I presume that in giving your thoughts to this topic, the very first reflection you would make would be to examine the nature of your opinions as to the primary or cardinal nature of the diseases that you denominate erysipelas, and childbed fever. If, upon so doing, you should determine that my words are quite erroneous, and that childbed fever is not a primary inflammation followed by a reacting fever, but a cardinal fever complicated with a peritonitis, &c.; and if you should resolve that erysipelatosus inflammation is but an accident or co-sign of a primary febris erysipelatosus, then I see no difficulty in your adopting the sentiments of some, who say that childbed fever and erysipelas are one and the same, or, rather, are results of one and the same morbid cause. I need not say that, in such a view, I cannot agree with you; and that, regarding the erysipelatosus fever as a result of the erysipelas, I can no more conceive of it as a contagious disorder than I can conceive of childbed fever as a contagious disorder.

396. There is an English writer, Dr. Philip Pitt Walsh, who

gave us a treatise, entitled *Practical Observations on Puerperal Fever, &c.*, which was printed in 1787. Though a very incompetent practitioner, as I conclude from his book, he was a very sincere observer and reporter of what he presumed to be the truth. As a full believer in the cardinal nature of the fever, he could not but be an earnest contagionist of the most strictest sect. Dr. Walsh does not agree with the writers, that erysipelas and child-bed fever are one and the same in cause. "If," says he, "we allow that erysipelas can only come on a surface covered with cuticle, or its continuation, which cannot extend to the peritoneal coat of the abdominal viscera; and with every accurate observer, that erysipelatous inflammation is not, when pure, attended with the formation of pus, the morbid exudation of coagulable lymph, and the consequent agglutination of neighboring parts, we shall soon perceive that something more than mere erysipelatous inflammation occurs here," p. 18. Dr. Walsh could not believe that puerperal fever is a "disorder, *sui generis*, confined to lying-in women, but merely an unusual form of a very common disease, and is, in reality, no other than the common infectious fever complicated with a more or less extensive inflammation of the peritoneum," p. 18. How could a man, holding such opinions, not be a contagionist as well as a denier of the erysipelatous identity of our malady?

I perceive by a late number of the *Medical Examiner*, that Prof. Simpson, of Edinburgh, has recently put forth the opinion, that erysipelas and phlebitis are diseases in the "same category as puerperal fever;" and I am free here to acknowledge the satisfaction with which I thus behold the gradual development of a sounder pathology of many diseases by means of a closer attention to, and acknowledgment of, the concern of the bloodvessels in them; and I confidently trust that the day is not remote, when the claims of the *membrana vasorum communis*, or endangium, as one of the chiefest seats of the lesion in many affections now wholly misinterpreted, shall be fully established and universally admitted. But while it is quite true that erysipelas is endangitis of the capillary vessels of the *corpus mucosum cutis*, that truth does not make it true that peritonitis is erysipelas, any more than it is true that the inflammation of smallpox is identical with that of measles or scarlatina. All inflammations, being essentially

forms of endangitis, or affections of the vasa vasorum, it would seem a very difficult thing to limit the range of such inflammations by computing the diameters of the vascular tubes that happen to be the seats of them. In larger veins, or in the more delicate vessels, they should be qualified as phlebitis; in the capillaries of the corpus mucosum cutis they would be erysipelas, scarlatina, measles, varicella, erythema, molluscum, and the great variety of cutaneous inflammations and exanthemes, each of which would present characters peculiar to the operation of the peculiar cause. In the serous inflammations, we should have pericarditis, pleuritis, and peritonitis, while the inflammations of the mucous membranes, whether respiratory, digestive, or genito-urinary, would also be, in principle, endangitis of their special capillary systems. To say, then, that phlebitis, and erysipelas, and puerperal peritonitis—much more the whole varied group of diseases comprised under the denomination childbed fever—are one and the same in cause, is going further than any man has a right to go, and further than any one would go, should he take a dispassionate and critical view of the cases.

As for me, I am not prepared to say that the epidemy might not have power over the nervous mass so to qualify its operations as to determine, in one individual, an inflammatory attack of the corpus mucosum of the skin, and in another, an attack of inflammation of the serous coat of the belly; but while I say this, I am very far from admitting that the facts are so; on the contrary, I do suppose that neither you nor I are possessed of any positive knowledge to warrant us in so saying. If it be true that the special malaria, or the epidemy which provokes so many attacks of childbed fever in epidemic seasons, does also occasion those inflammations of the corpus mucosum of the skin called erysipelas, I am very sure that we do not as yet know it; and our whole duty, at the present is, to collate facts already ascertained, to observe with care all the new examples presented in our clinical experience, and leave it to our successors in a future age, to deduce the law of the case. For the present, therefore, I am opposed to these vain imaginings of facts and laws, and am not capable to believe that there is any greater identity of putrescentia uteri and metror-peritonitis, than of smallpox and yellow fever. If, in a surgical hospital like Hôtel Dieu, where, accord-

ing to Tenon, the air was absolutely poisonous, a wounded man should be seized with erysipelas, and an inlying woman with peritonitis, or metritis, or phlebitis, I shall not, on that account, confess that in either of them the disease is zymotic; any more than I should do so if the woman's illness were produced by laceration, contusion, or other accident, and the man's erysipelas brought on by intemperate habits, or irritating dressings to his wounds.

397. The error against which I most desire to guard you, is that of falling into ontologism. I mean to say that I would warn you against adopting the opinion of a *materies morbi*, which I spoke of already. Should you take up the notion that there is, in cardinal fevers, some entity floating in the blood, or mixed up with the tissues and juices of the body, and which maintains the diseased actions until it is itself eliminated and cast forth by the emunctories, then I am sure you would be ever astray in your therapeutical indications and ministrations, and be ever fighting with ontological shadows, instead of directing all the force of your intelligence and skill to the removal of a disease of the living solids. You are ever in your practice to oppose and cure affections of the living solids; it is there you shall meet and combat the material enemy of your patients' health or life, and not some hypothetical ens, floating in the air of the atmosphere, and entering by direct contact into the recesses of the body, or wafted from great distances as halones or nebulae, to poison and change the physical substance and faculties of the victim within whose body it has taken up its abode.

398. Erysipelas is a disease of the skin; and although in some instances it does take on a phlegmonous character by extending perpendicularly downwards into the connecting areolar texture, it does so only by accident, and not as a normal process of that special phlegmasia. Erysipelas is therefore a membranous, but not a visceral disorder, and one of its chief characteristics is found in its propensity to expand its areas of phlegmasia far and wide over the plane of the membrane. Puerperal peritonitis, pure and simple, is also a membranous disease, and possesses the same propensity to expand its areas over the entire plane of the peritoneal membrane. In this, erysipelas and peritonitis are alike; but erysipelas and metritis are not alike. Peritonitis is also like

erysipelas in this, that it has a tendency to plunge or descend vertically in the basement textures of it, and so destroy the epiploon, or gangrene the bowel, or produce ramollescence of the exterior stratum of the uterus, or exo-metritis. In erysipelas, this vertical plunge or down-sinking of the morbid status of the corpus mucosum often carries it quite through and below the corium, and far down into the substratum of areolar tissue, where it may become either phlegmonous or oedematous erysipelas, as the case may be.

399. In like manner, when you shall hereafter examine the mortal remains of individuals who have died with pure childbed peritonitis, though you shall not, in general, observe any other than the results of a purely membranous inflammation, or inflammation of the peritoneum, yet in some specimens you may find the epiploon softened and suppurated, the ovaria reduced to a pulp, or the outer stratum of the womb completely reduced to a state of ramollescence or softening. In so far, then, as I have drawn a parallel between the two disorders, you discern a very great similarity in them; but I would not have a Student of mine leap to a conclusion, or make up his mind concerning a great matter upon some sudden impulse.

400. If you will be ontologists, then you may say that erysipelas and puerperal fever are identical; but your saying so will never induce me to believe that a man with erysipelas has a childbed fever; or that a woman with only a childbed fever peritonitis, pure and simple, is laboring under an attack of erysipelas, or St. Anthony's-fire in her belly. Don't you see such a conclusion would be as ridiculous as it appears under my statement of it?

401. Besides, why should you vex yourself with this foolish question at all, when you are already so well informed in your profession as to know that childbed fever is not puerperal peritonitis and nothing else; and that peritonitis, in numbers of the cases, does not come in question at all, the disorder being pure womb-phlebitis alone, or a dissolving inflammation of the inner stratum of that organ alone; or an oophoritis, &c. &c. If you must insist that erysipelas and childbed fever are one, then pray leave out of question all the pure metritis and phlebitis, and confine your alliance between the two, to the serous and dermal

identides, if they must be so considered. For my own part, I could never discover any real motive for discussing this question in any way, except, perhaps, it were for the purpose of giving a handle to such, as, by means of it, desire to demolish all those who can discern neither the philosophy nor the charitableness of the contagionist party, who, themselves believing the contagion of erysipelas, do not hesitate to proclaim that you may give a woman childbed fever by taking care of her in her labor, after having visited or handled an erysipelatous patient or subject; thus creating, by force of a vivid imagination, an ens out of nothing; and then endowing it with power to produce a disease like itself—itsself, meantime, being an ens in *posse*, not in *esse*. Do I, then, advise my Students to soil their hands with the putrilage of an erysipelatous ulcer, and go without precautions of cleanliness to deliver the lady next door? God forbid! As soon would I advise you to plunge a wounded hand into the cavities of a patient dead with peritonitis, and so run a most dangerous and often fatal risk from dissection wounds. If you carry your impure hands, and inoculate the abraded genital surfaces of a woman in labor, with the poison of the cavities, you will be as apt to give her a dissection wound, as you would be to have one yourself if using a cut hand in your necroscopic researches. You would poison her as by the bite of a rattlesnake, and so make her sick, and her sickness would be prone to assume the characteristics of a childbed fever. Is this your contagion? Far from you, Students of my Class, be the silly thought!

C. D. M.

LETTER XV.

DESCRIPTION OF A CHILDBED FEVER CASE.

402. I HEARTILY wish I were possessed of the power of describing in such a perfect degree that I could write down in this Letter a circumstantial account of the appearances, incidents, feelings, and all the circumstances of childbed fever cases, and do it in such a way that, reading my relation, you would seem to be present and witnessing the whole scene; but this I cannot do. Many writers have given us the bare histories of cases, but not one, so far as I know, has really painted it for us, unless, indeed, Andrew Low's wife has been brought before us bodily, as she was, in her illness, by Alex. Gordon; and unless, also, some of the stories in Robert Gooch's work possess so graphic a character that, in reading them, one seems to be partaker in the incidents and sentiments he portrays. But Gooch was one of our best writers, and even he has failed.

Many critics, who dislike my books, clearly declare that I cannot write intelligible English, and would almost make me believe that my words are all gibberish and pedantry. If Gooch even, who was one of the ornaments of our Medical Literature, could not fitly describe a case of childbed fever, how, then, should I hope to compass so difficult a thing? Yet, I once attended a young lady, the mother of three most beautiful children, who sent for me in a severe winter's night, to take care of her in her labor. She was a very small and delicately formed creature, of the most cultivated understanding, and so thoroughly well bred that you may readily suppose she would go through the pains of her parturition with the fewest possible expressions and acknowledgments of distress, since perfectly well-educated persons always behave so.

403. The labor was rather a trying one, in consequence of the abnormal action of the womb; for the pains, being irregular, and serving at times rather to restrain than to advance the progress of the presenting part, brought frequent disappointment of the hopes they raised. An experienced accoucheur, meeting with a ease of the kind, and unable clearly to explain why such non-conformable action of the several parts of the womb should occur, will, for the most part, be inclined to suspect that the placenta is abnormally attached, and to fear, therefore, that he may have to deal with an hourglass contraction before the deliverance is completed.

This proved to be the case in regard to my patient. There was at the time some slight epidemic disposition to childbed fevers in Philadelphia, which increased my anxiety for the lady.

When I found that the hourglass prevented the escape of the placenta, as it always does, I also knew that I should be obliged to separate it with my hand, as I have always been, and so it happened. The contraction in the uterus was slowly and gently overcome by the hand, and the after-birth removed without much pain or delay; certainly with no exclamations or the least impatience on the lady's part, and without any hemorrhage. The lochia were favorable, the milk began early to come into the breasts, and the pulses were good; but, on the third day, at an early hour, she had a rigor, followed by hypogastric pain, incipient tympanitis, dorsal decubitus, hot skin, cephalalgia, and pulsations at 120 per minute. The uterine discharge became rapidly less, paler, offensive, and then died away, except for a dark satin left on the guard-napkins, which were in the highest degree fetid. The breasts also became flaccid. She was bled, the bowels were well moved, and she was kept, after that, under the influence of tartrate of antimony and potassa. I have no doubt the peritoneum was inflamed at the time she was bled, nor have I any doubt the inflammation was cured; but there was a deeper-seated, immedicable, phlegmonous nucleus of disease in the womb and in its veins, that remained unresolved by the treatment, and that must have some other termination than by resolution.

404. I need, perhaps, say nothing of the moral state of the family and friends of the lady; their anxious, scared looks, as they waited on the stairs, or in the hall, or at the door, to learn

"What of the sickness?" "Is there great danger?" "Will she be well again?" nor can I tell you how that one great heart of all who cherished and loved her, throbbed so, that we might almost seem to hear its throbbings in many a breast that was aching. The domestics walked softly, and spoke in whispers; you could hear no loud voices in that house.

What shall I say, in a weak vain hope of portraying the feelings with which the physician approaches the bedside of one so perilous; what of the deep inner conviction with which, on glancing at the countenance, the decubitus, the gestures; or on touching that hurrying artery, or on perceiving that morbid heat or coolness; on hearing those expressions that lift him, on the one hand, to the height of hope, or whisperingly tell that the vein is inflaming more and more, that the purulent infection is begun, and that his art, like his wishes, prayers, hopes, are all alike in vain?

The pulse had fallen a little, and, the sharp pains being gone, she was deemed much and hopefully better; everybody thought so—but the pulse would not go down in its frequency below 120, and the skin, though not so heated as before, was still warm. The tongue was white and moist. Various applications, in the way of stupings of the belly, cataplasms, anodyne enemata, doses of Dover's powder, portions of calomel with kermes, &c. &c., made no real impression upon the disease within the uterine veins. Yet, though the pulse remained too quick, and it was evident the lady was very ill—not really better, still, she would sometimes meet me with brilliant smiles and cheerful sallies of conversation, and would even rise up in bed, as though a little intoxicated or out of her head with her gayety. She was unnatural in her naturalness. If she slept, it was but by snatches and with light flying slumbers, disturbed occasionally by her own murmurings. The countenance was usually animated by a peculiar lively look, though she was pale.

At times, however, there would rest a cloud upon her countenance, and she was mournful in her physiognomical expression. The keenest anxiety now pervaded all hearts of those who were about her and loved her; for, though she did not converse or look like a person very ill, and doomed, there was a deep-seated conviction in all opinions that the spirit which animated this charming woman was soon to be recalled to its home beyond the skies, leaving

the body in this dark and mourning world, a lifeless, inanimate form of clay. Who is he that can paint the trembling anxieties, the flitting hopes, the more permanent terrors, the heart-swelling pity and tenderness that wait on such scenes? By and by she would sleep, and filled all with the hope that such benignant slumbers could not but be the harbingers of good. Then she would awake from her sleep and ask if she was better—would she recover again? and this with sweet smiles, amidst which the doubt was nestling. She rose in bed and sat up, combed out her hair, and bound up her tresses in tasteful arrangement. She sang the sweetest and softest harmonies, and looked so arch and so bewitching, that one could not avoid the suspicion she was acting a part. No actress could be so like a sense-stricken Ophelia.

Meanwhile, the pulse at 120 to 130, the rapid absorption of the fat and the growing marcor, and the viscid sweats on the arms, kept marshalling the way to the grave. Little complaint was made of pressure on the hypogaster, though on firm contact pain was felt. The tympanitis was over. A small diarrhoea began to grow troublesome; and now and then very violent fits of excitement, with restless jactitation, showed that the pus-corpuscles were poisoning the sources of life in the nervous centres, to which they were transported with the blood. Then came calm tranquillity, the greatest gentleness, scrupulous politeness, and careful attention to her personal appearance and array. But the pulses ever beat faster and weaker. Eructations of gases; then of colorless fluids; then porraceous vomitings; then darker ones, and at last black vomit; lessening pulses; increased respiratory efforts; cooling hands and feet, that turned bluish in their cyanosis; until at last, a death-like coldness overspread the limbs, and the icy king advanced slowly to the conquest he made over what was most enchanting among educated, elegant women. Stupor, lethargy scarcely to be roused, then deep coma, and at last death—rest for the victim, and tears and suppressed moanings for the bereaved. Why say all this? Why, a physician to practise Midwifery must be made of stone if he would not feel on these occasions; and, if he have a heart of flesh, he surely deserves the sympathy of all good people when he turns away a baffled man, after faithful, well-conducted efforts to save one whom no art could save.

405. I have described one of the scenes of fatal childbed fever.

I have not done it merely to excite your sensibility, but in order that you might, if possible, see how it is that, in one of those cases, even when early called, and applying conscientiously and wisely too, all the resources of your professional skill, you shall be disappointed of any good fruits from them, because the deep and peculiar seat of the disorder in the uterine veins, makes it impossible to determine, in cases of metro-peritonitis, whether or no your early administrations have destroyed the monster at a blow. It is, as Legouais told you, like fighting the Hydra; when one head is cut off, there is another to rise more fierce and furious. I do believe, however, that in those cases that consist mainly in the peritoneal form of the inflammation, you will cure the far greater majority of your patients whom you shall visit very soon after the formation of the attack; and besides these there are many worse ones to be rescued by science and skill, governed by courage. For God's sake, then, omit no means that may serve to early notify you of the assault, and never stop short of the greatest precision and certainty in all your diagnostic proceedings.

LETTER XVI.

DIAGNOSIS OF CHILDBED FEVERS BY INQUIRIES AS TO PAIN, RIGORS, THE BREATHING, SORENESS OR TENDERNESS OF THE BELLY—THE MAMMARY GLANDS AND MILK—INTERMITTENCE OF THE PAIN—THE BLADDER OF URINE—IS IT A URINARY OR A UTERINE PAIN?—THE EARLY PHLEBITIS NOT PAINFUL—DATE OF THE ATTACK, AND THE TIME ALREADY ELAPSED—MILK FEVER—M. CHEVILLARD'S CASE—REMISSIONS IN THE CASES—ONE DESCRIBED BY TONNELLE—MILK FEVER WITH AFTER-PAINS, OR INCIPIENT CHILDBED FEVER?—TOUCHING, AS A DIAGNOSTIC PROCEEDING IN CHILDBED FEVER—THE TYMPANITIS—COMPARE PAINS OF PHLEBITIS AND PERITONITIS—THE MIND—PUS—INTOXICATION.

406. A PHYSICIAN entrusted with the care of a lying-in woman, ought to be so well versed in the nature of all acute diseases that he should almost never err in his diagnostications; and I think there is almost no disorder he could be called to treat, in which it is more important that his diagnosis should be early and distinctly ascertained, than in these.

407. Childbed fevers, however, are, perhaps, not always to be at once ascertained, because there are other affections of puerperal women, that possess features strikingly similar to them. For example, one of the most considerable marks of childbed fever is pain in the iliac and pelvic regions, following a chill, and accompanied with a frequent pulse and soreness of the hypogastrium on pressure, and unwillingness to move for fear of pain in so doing. But there is many a woman whose womb, after delivery, becomes very sore if moved by a hand placed on the hypogastrium, or by a cough or by any change of position; and in these very cases the woman hardly shall avoid suffering from after-

pains, as they are called. Now, whenever the muscular tissue of the womb contracts forcibly, the womb being thus sore or sensitive, the patient experiences the greatest distress, and some women are liable to a return of the contractions every five or six minutes.

408. How will you distinguish these pains from those of a peritonitis? It is to be expected that the nervous centres, feeling these impressions, shall send back a stream of reflex innervations, and so augment the number of the respirations as well as the animal heat. Chilly fits are likely to attend these occurrences, and when you come in to make inquiries, how shall you decide? Is there an area of phlegmasia already laid, or no? that is the question to be settled; for, if there is such an area, then you surely should adopt convenient measures for its cure; and if not, then the suffering of the woman may be probably relieved by a purely anæsthetic method, less costly to her health and patience.

409. The doubts and uncertainties that wait upon such circumstances, are extremely painful to the quick-minded physician, who, unwilling to take severe, and perhaps unacceptable measures, balances between two opinions when he is most desirous to know the absolute truth. I could not if I would, tell you what pungent distress I have experienced in this way on many an occasion, depriving me of all comfort and driving sleep far from me through the perturbations of my soul. I endeavored to relate such an incident in my *Letters on Women*, &c., 3d edit. p. 559.

410. I have striven earnestly to carefully ascertain in such cases, whether the pain, or rather the soreness, were permanent or no; for I supposed that where a red, hot, painful inflammation has been really formed, there could occur no moment of time in which the soreness or painfulness would not be detected by careful pressure with the hand, at the same time calling the patient's attention to the objects of the inquiry, and so, deriving good inferences from her replies to questionings. In what are called after-pains, the globe of the womb becomes hard as a stone, and will not then bear the least pressure without complaints from the woman; but, as these contractions, like labor-pains, rarely last more than twenty-five to fifty seconds, and as the womb, after the contraction is over, becomes soft and flaccid, and painless, the woman

can now bear to have the lower belly and the iliac region freely moved by the hand, and that without any complaint whatever. Not so when the womb or peritoneum is really inflamed. This is a good diagnostic sign or difference, for interrupted soreness and pain leads to inferences different from those produced by uninterrupted pain and soreness, or tenderness of the belly.

411. You ought not, however, to confide absolutely in this differential diagnosis; for it is quite true, as I have said in a former section, that an inflaming vein does not, at first, prove painful; but when the basement-tissue, on which it rests, has begun to participate in the inflammatory turgor, then it becomes exceedingly painful. There is, therefore, some liability to mistake in the beginning, if you rely exclusively on the patient's answer to your inquiries: "Does this hurt you?" "Is this point sore or painful?" for the constitutional reaction may, all the while, have its source in the painless phlebitis; painless, I say, except when the muscular organ of the womb, by crushing under its spasmodic force of contraction, all the textures it incloses, brings out evidence of the secret or hidden painfulness, now rendered obvious by moans or cries. Take care that you do not make a serious mistake which might lead to her destruction, by giving you a false indication of treatment. I shall hercafter (section 496) tell you that the loss of time is equal to the loss of a life.

412. As to the chill, the quick pulse, how are you to infer concerning them?

You should minutely examine the state of the breasts, and the milk secretion. Does the rigor, and does the hurried pulse, &c., refer to a constitutional disorder called milk-fever? If the breasts are large, warm, tender to the touch, and filled with undrawn milk, you have a very legitimate inference from those circumstances to the constitutional disturbance; and it is generally though not uniformly the case that, as soon as the womb or the intro-pelvic peritoneum begins to give way to diseased action, the breasts shrink, become flaccid, and secrete little or no milk.

You could hardly suppose, gentlemen, that in a furious inflammation of the parts contained within the pelvis and abdomen, and when the circulation and the nervous forces are so violently shaken and disturbed, the quiet, gentle operation of the mammary glands

should go on with an even tenor of their functioning. On the contrary, the extreme disorder of all the parts below the diaphragm, might be expected to exercise a kind of cumulative life action, on the principle that *pars dolens trahit*. To find, therefore, a full breast of milk in a woman laboring under severe childbed fever, would be something strange. Even when the attack of the inflammation does not come on until the breast has already become fully distended, and rife with abundant secretion of milk, the production becomes lessened soon after the attack of the phlegmasia, and rapidly ceases altogether, leaving the breasts quite flaccid and shrunk. Walsh says of his patients, p. 9, that they appeared in some of the women as flat, as the generality of persons of the male sex, even of a spare habit, are found to have them.

Your studies in this line will show you that hundreds of our brethren have used all the means in their power to provoke the breasts to the fulfilment of their lacteous functions, and to invite back the secretion when it has failed. For my part, I expect it to disappear, and I do not believe anything we can do can prevent it. Neither, indeed, do I suppose that if the secretion continues and the woman recovers, we ought to attribute the escape to the continued production of the milk, but rather that we should allow for the continued secretion, on the hypothesis that the disease was not violent enough either to suspend the mammary action or to destroy the life of the woman. Attention to the breasts, therefore, seems to me less important than it is usually imagined to be.

Though this loss of the milk is one of the most common incidents in the practice of obstetrics, and one, therefore, familiarly known to all physicians, I will take the liberty to put down here a very remarkable relation of one that was deemed so important by MM. Hallé and Vicq D'Azyr, as to be reported on by them for the Royal Society of Medicine. It is to be found in the *Hist. de la Soc. Royale de Méd.* vol. viii. p. 261. The case occurred in the practice of M. Chevillard, physician at Lons le Saulnier, probably in 1784 or '85. The date is not given. You will please notice in the relation the then prevalence of the milk dogma.

413. "The patient, who was nursing a child, had for some time been troubled with a double quartan ague. She weaned the child

without taking any precautions, because she suspected that she had become pregnant again. The milk coagulated in the left breast, and the engorgement became painful. During five or six days, many applications, good and bad, were made, but there was great surprise one morning, upon taking off an emollient poultice mixed with saffron, to find the swelling entirely gone, and from that morning the tertian also wholly disappeared. The abdomen had become meteorized and excessively painful; the pulse hard and small. There were frequent faintings, accompanied with efforts to vomit; both stool and urine were suppressed, and the use of the catheter became necessary. The administration of the ipecacuanha, which, notwithstanding the pregnancy, and certain other unpleasant circumstances, was repeated four times, removed in succession some of the symptoms. At the third dose the urine became more abundant, but the right hypochondrium became swelled and painful. The loins, the hips, and the thighs were successively attacked with pain after the hypochondrium became relieved, and then the double tertian resumed its course. The woman slowly recovered, and was, in the end, safely delivered, and suckled the child, notwithstanding the paucity of the milk and the unhealthy state of her bowels, not yet wholly recovered from." MM. Hallé and Vicq D'Azyr remark upon the strangely sudden metastasis of milk, that was supposed to be coagulated within the breasts, but which, nevertheless, "transferred itself bodily in a few hours, into the abdominal viscera." They doubt, however, that what was considered coagula, was nothing more than knots of milk vessels, over distended. Yet both these eminent gentlemen regarded the whole affair as a case of milk-metastasis.

414. You may interpret the above case according to your own judgment; as for me, I have related it to you for the purpose of exemplifying the disposition of the breast to lay aside its vital activity, whenever some other more important organ or viscus calls around itself the reacting forces of the nervous and sanguine system. I wish you to read in it a lesson, teaching you that, in those doubtful diagnoses, you ought cautiously to consider the state of the breasts, and draw from them inferences, which, added to others, become at last the accumulated evidences

if not proofs, that the pains, heat, soreness, immobility, &c., &c., are, or are not dependencies of areas of phlegmasia within.

415. I ought to put you in mind that you are occasionally to treat cases of childbed fever that tend to embarrass your judgment by a set of remissions which prompt you to suspect them of possessing a malarious or intermittent origin and nature. These remissions often become so considerable as occasionally to induce the attendant to look upon the disease as conquered, just as happens in yellow fever at the close of the paroxysm; but the calm is treacherous. When the paroxysm is renewed, it will generally be found accompanied with evidences of purulent infection of the blood. Here is one of Tonnellé's cases that I cite for you from page 365. Marie M., æt. 28, good constitution, was attacked, three days subsequent to a natural labor, with symptoms of serious puerperal fever. The disorder, which was treated by means of copious topical depletion, soon showed signs of amendment, and on the 8th day seemed to be completely cured. But there soon came on headache, buzzing in the ears, agitation and depression, and by turns a little delirium. The 13th day she had chills, and the pain in the abdomen, that had wholly disappeared for a long time, came back again. These signs of relapse were combated by means of mercurial frictions, of two ounces, twice a day; she grew better under it, and seemed again convalescent; began to get up and to take food. Nevertheless, she was a little feverish every day, and grew thinner. On the 22d day, the legs were enormously infiltrated. On the 29th, the abdominal pain, vomiting, and fever returned, and did not again cease until, on the 31st, she fell into a state of extreme prostration, and died. There was pus in the peritoneal sac, with false membranes. The womb was perfectly natural. The hypogastric veins contained pus. The iliac and part of the cava had a clot, with pus in the centre of it.

416. But, after all, you have a very sure resource for satisfying your conscience in those instances wherein you cannot clearly determine whether the disease is only milk-fever with after-pains, or whether it is really incipient childbed fever; and that resource consists in your power to act. I have many times, amidst hesitations, doubts, fears, and the extremest anxiety, resolved them

all by taking the safest view of the question, which I did by proceeding as if I were healing a case of childbed fever. That is to say, I made my venesection, *ad jugulare febrim*, and rested in an ever unresolved doubt, whether by doing so, I had cured my patient of a milk-fever, or of a childbed fever. I advise you always to take the same method, believing that this is the best practical direction I can possibly offer you, under such circumstances. If the milk-fever is violent enough, and attended with neuralgia of the uterus sufficient to make you doubt of the diagnosis, it will, *ipso facto*, present motives sufficient to induce you to prescribe a proper venesection, and that is the safest and best thing you can do.

417. Alas! you will not always come in early to visit the patient stricken with these dreadful disorders. You may be called, in consultation, after many days of the disorder are already past, and when great doubts still exist concerning the nature of the malady. A very excellent resource for clearing up your doubts will be found in referring to the state of the bladder of urine—whether it acts well, and without pain or hesitancy; whether its containing power is normal, or whether it expels its contents as soon as half an ounce or an ounce is accumulated within it. The examination of the parts within the pelvis, gives you a very good rule to judge by, because if there is inflammation within the pelvis, you can thus reach with your hand, the sore and painful organs, and in a sort thus interrogate them and find the answer.

418. Tympanitis, or ballooning of the belly is an invariable symptom, but it is greater or less in different cases and times. You ought to expect to find a smaller and softer abdomen in metro-phlebitis than in peritonitis.

419. The pains of metro-phlebitis, after the first few hours, are not so great as those that attend on inflammation of the peritoneum.

420. In peritonitis, pure and simple, the mind is clear; the nervous system not being disturbed by the presence of pus-corpuscles in the blood; the woman recovers, or dies, without those hysterical or rather hysteroidal and even maniacal symptoms, that invariably mark a purulent infection of the blood.

421. The respiration is less distressed by the course of a metro-phlebitis, or a putrescentia uteri, by reason of the less soreness, and less interference with the play of the diaphragm. The diaphragm may be compared to the piston of an engine, that moves in the cylinder of the trunk of the body; its descent draws in air from the trachea, its ascent allows of its escape from the lungs. If this respiratory piston, then, in descending in the cylinder of the trunk of the body, must compress beneath it the inflamed and inflated masses of intestine, it is evident the motion will be modified by the sensations and necessities of the patient attending the acts. She will not breathe freely; she dares not, she cannot. Not so, when the disorder is confined to the womb alone, and that situated in the cavity of the pelvis, out of reach of the compressing, crushing power of the descending diaphragm.

422. In attending a case of questionable diagnosis, you will, not rarely, observe states of the mind, physiognomical expression, speech, conduct, gesture, that shall lead to expect an hysterical state. Ah! let me already alarm you as to this curious condition of lying-in women. Without doubt, inasmuch as all hysteria proceeds from the reproductive system as its radiant point or source, you will meet, in practice, with exquisitely-marked instances of hysteria related solely to some suffering state of the uterus or ovaria, &c. But the alarm I wish to awaken in your mind is one connected with the very great probability that hysterical or hysteroid affections, in women recently delivered, and assailed with fever, are really the exhibitions of that curious influence that pus in the blood, or pyæmia, exerts upon the nervous system. Here is, then, a source of difficulty in your diagnosis; for if it be that the signs are hysterical, merely, there need, in general, be no resulting alarm; but if they are hysteroid, and if they do arise from purulent infection, then the prognosis is most unfavorable; not fatal, but most unfavorable; since, whoever she may be that exhibits them, during a fever with pulses at or above 120, she will be more apt to die than to recover.

423. You should not disregard the just reflection that childbed fevers do, in the majority of cases, consist, in part, of metro-phlebitis. M. Tonnellé says, *Archives*, xxii. 1830, p. 355, that "suppuration of the venous and lymphatic canals is an alteration

whose frequency is so great, that I meet with it in two or three out of every five cases of puerperal fever."

424. I have, on various occasions, treated patients ill with fevers, whose real nature was only disclosed to me when, after many days of attendance, I observed these hysteroidal phenomena to be presented by them. It was then that I knew them to be cases of pyæmia, or, in other words, endangial inflammations that had proceeded to the stage of pus-production. In all the individuals, male or female, that I have attended in mortal illness, from wounds of veins in venesection, I have invariably noticed this kind of hysterical intoxication, caused by purulent infection of the blood. I advise you, therefore, to keep watch for these appearances, and thereupon to take such measures as you may. I fear that those measures, however, will generally prove of little avail. I say they will generally prove unavailing; but I have conducted not a few women through such great dangers. One of these ladies was delivered, in a very tedious first labor, with the forceps. I did not observe any particular difficulty in the operation, which she bore without much complaint or repining. A few days afterwards she evidently labored under pyæmic fever, seated, I apprehend, principally in veins lying in the ligamenta lata of the womb. Her life was long in great danger, but at last she began to form abscesses on the lower extremities, on the forearms, between the thumb and forefinger, above the elbow, on the inside of the left thigh, on the region of the sacrum and loins, &c. &c., so that I visited her almost daily for a period of seven months before she was fully recovered. In observing and healing these abscesses, some of them of vast magnitude, I never could persuade myself that the major part of them were not abscesses of transportation, that is, pus brought to the part and poured out there, and not formed there originally. She went to sleep one night with her left hand perfectly well; at my visit on the next morning, I found an abscess, big as an English walnut, round and plump, distending the folds of skin between the left thumb and index finger. I discharged the collection by means of a lancet, and the part was cured in twenty-four hours. The quantity of pus that this lady excreted during her illness was very great indeed. Since her recovery she has enjoyed excellent health.

425. I believe I need say nothing further concerning the disappearance of the lochia and the milk. The presence or absence of those secretions will afford you some items of a collective diagnosis, which it were tedious and needless to go over with in this place.

426. Think of these things, young gentlemen—treasure up all good means of diagnostication—for upon your diagnosis must rest, as its sole basis, the prognostics as well as the therapeutics of your cases.

C. D. M.

LETTER XVII.

PREPARATION FOR LABOR.

427. I WISH to say something in these Letters concerning the methods by which you ought to hope that the attack of child-bed fevers may be prevented for women under your charge.

428. All pregnant women ought certainly to be held as obliged, by the very fact of their imminent accouchement, to act circumspectly as to diet and exercise, which, being conducted with prudence and wisdom, should allow them to expect a favorable getting up after the birth of the child is completed.

429. Labor, for many women, is a great and terrible conflict; it is attended with enormous efforts and followed by great fatigue. No woman, who has a little common sense, would be willing to march up to such a conflict with the fullest and most brilliant health. She would prefer to be rather delicate than strong, for there is never to be dreaded any lack of power, but only excess of resistance is to be feared. If the resistance is great and obstinate, the needful power must be developed, and that costs, sometimes, more than the health can bear. These observations I conceive to be applicable to all puerperal women, in all seasons, and they should teach the necessity of attending to the amount and kind of food and drinks, the state of the bowels, and the condition of the blood. The woman may require more food than she takes, or less; or she may require astringents, or aperients; or she may be in need of wine on the one hand, or of free blood-letting in opposite circumstances.

430. There is not, and there cannot be any specific drug that you may order as a sure antidote to the cause of childbed fever. Your duty will be done whenever you shall have so ruled her conduct, as to lead her to the fulness of her time in good health, by which I mean neither excessive plenitude nor great weakness.

431. It was formerly, in this quarter, almost an invariable rule for pregnant women to be bled once or twice at the seventh or early in the ninth month, as a prophylactic. That good custom is now generally abandoned. I presume that, where you may have reason to fear any *post-partum* bad effects of repletion, plethora, a tendency to heats, or inflammations, you will resort, as your best prophylactic, to an occasion of bleeding for the woman. There is excellent reason to believe that one or two venesections, well timed, may enable the woman to pass through the pains and perils of childbirth with greater ease and less risk than those who refuse to adopt such a precaution.

432. There is a general tendency in women far advanced in gestation to a costive state of the bowels, or, rather, to excessive accumulations of feces in the great colon. In pretending to give advice of a prophylactic kind, you should not lose sight of this tendency, but give directions for its removal.

433. The air of the place which the woman inhabits is to be inquired into. It may be too damp, or too hot, or insalubrious on account of marshes or standing pools in its vicinity. Even the chambers of the house ought to be considered, relatively to the season of the year, and the patient directed to take an upper or lower, a south or north room, according to your judgment of its comparative salubriousness. Precautions of this kind it is worth while to attend to for all cases, but the propriety of them in seasons of epidemics is not to be called in question.

434. Where there is some alarm of the sort, a prudent woman, in ordinary health, ought to prepare herself by a proper regulation of her diet. There are many who refuse to eat meat for thirty days, and some for only fifteen days before term, thinking thereby to obviate a supposed inflammatory or febrile diathesis, developed by their gravidity. If there should be some difficulty in persuading a woman to leave off her meat diet for so long a time, you may come to an equally useful result by ordering a meat diet for every other day only. Thus, if you allow meats every alternate day, for twenty days, your patient would be on a vegetable diet for ten out of those twenty days, and with less reluctance or objection on her part. Many of my people follow this rule of alternating days, and it is a very excellent one, which I advise you to adopt in your practice.

435. You have seen, in a former page, how important it is deemed that the milk secretion should be formed and maintained as a means of securing the patient from the danger of a childbed fever. You cannot be too careful, on this head, in recommending the early application of the child to the mother's breast. It is true that the granules of the milk-glands will, some time between the forty-eighth and seventy-second hour after the labor is over, commence to excrete the milk; but it is no less true, that the normal irritation of the nipple, by the mouth of the new-born child, does greatly favor the early coming and readier flowing of the nutritive element. I should consequently regard it as an essential duty, to cause the infant to be presented, as soon as the woman's state should warrant it. Moreover, the nurse should be instructed to keep the breasts properly covered; for there is little doubt, that cold and damp air, or dresses imperfectly covering the bosom, do prove repercussive of the mammary secretion.

436. The foul excretions of the womb, gathered in the lower part of the vagina, and retained about the perineum and vulva by napkins or receivers, not frequently enough changed, are observed to be pernicious to the health. Every such patient should be scrupulously cleansed by the nurse at least twice a day in cool, and oftener in warm weather. It may seem strange that I should give such an advice, supposing that no decent woman would require such counsel; but you can yet little know how much of barbarism exists even in polished society, or how stupid mankind are in presence of time-honored opinions! There are many women who think it is as much as a life's worth to use any method of ablution for many days after a confinement. Not only so, but there are many very reputable people who believe in table-turning, spirit-knocking, homœopathy, and other things equally absurd, equally incredible.

437. Labor often produces an effect on the neck of the bladder that causes the woman to retain the urine, until a very large quantity accumulates in the cyst. I drew off sixty-two ounces for a patient some time ago. She was in dreadful distress, and though attended by her physician, no suspicion of the cause of her sickness arose until I came to her chamber. Do you doubt that over-distension of the bladder might determine the establish-

ment of an area of inflammation, to end in childbed fever? Take great care, then, of your patients, and never leave them without this needful instruction as to the state of the bladder of urine. It is amazing how stupid some people are on this point, and to what an enormous extent they do allow the bladder to overfill itself, with great pain all the while. When you come to them, and touch them, and ask, "is that a urinary pain—a pain to make water?" they at once answer in the affirmative, though so dull as not to have conceived of it before. There are some people in this world who do not know even how to get out of the rain!

438. Some authors think the bed, by overheating the woman and detaining odors about her, may provoke to attacks, and so, they order the patient to sit up too soon. I cannot adopt this view, and I am very sure many of my bad cases have been brought on by too early sitting up.

439. The chamber should be well ventilated, but the woman should be kept out of cold draughts of air. Here is an old distich:—

"If the wind blows on you through a hole,
Make your will, and mind your soul."

If the patient is very tired after labor, you may give her a half-glass of wine with water. It will do no harm if the pulse is right. The diet for the first three or four days is bread with tea, sago, or arrowroot jelly, and cold water to drink. Don't let anybody take oatmeal gruel until the milk has come abundantly, and the lochia which are checked at the coming of the milk are re-established. Drive out all chattering folk and bearers of bad news. If she is restless and suffers from after-pains, let her have an anodyne, and repeat the doses until she is relieved.

C. D. M.

LETTER XVIII.

MEANS OF CURE.

440. IN the management of cases of childbed fever inflammations, you are to employ what are called remedies, by which is meant some drug or medicine, or some application, or act that may give the disorder such a bias or direction as to cause it to cease and allow the woman to recover. And I do sincerely believe that your success or failure will in chief depend upon yourselves, and not upon any known tendency of the disorders to come, after passing through certain regular stages, of their own nature to a happy conclusion. There are many disorders whose tendency is to a cure, but these do not belong to the category of self-curing diseases; and it is to be believed that women attacked by them, and not cured by art, do, for the most part, certainly and miserably perish.

441. In coming, in this portion of my work, to advise you as to methods of treatment, I am more than ever struck with the incomprehensible differences of opinion I find among authors of all epochs and all countries. Many of those who are esteemed to be the highest authority, declare that these dreadful diseases are to be cured by a tonic and alexipharmic method, and that blood-letting is, in the main, a most dangerous thing, and unsuited, except for a few and peculiar cases. Other authorities having, it is said, equal pretensions to our confidence, aver that venesection is the principal remedy, and that it may be depended upon with great assurance of its safety and efficacy.

442. Are we not all physicians? How, then, are we like a house divided against itself, and how can we stand? The fact is, we do not stand; for we are so fallen in the esteem and confidence of the public, as to this particular matter, that they do not believe in us, and are frightened and panic-struck by the very name of

childbed fever, when it comes to interest them directly or nearly; and though they employ us, because they can do nothing else, yet they do not believe in us.

443. You have observed what great pains I have taken in these Letters, to lay before you a clear, undeniable doctrine as to the nature of childbed fevers; and I know not yet, whether I have carried your judgment captive, by means of my arguments and citations, or what views you shall take concerning a fitting treatment of such maladies. If you remain contagionists, putrefactionists, vitiated-humorists, &c., you will grope about among the bottles and drawers, filled with drugs in an apothecary's shop, for some specific thing called a drug or medicine wherewith to cure your childbed fever patients, and you will try to cure them Chemically, or Empirically; but if, on the other hand, you do regard the causes as consisting in inflammation of those parts I have told you of, then you will not put your trust in physic alone, but employ such measures as are best suited to oppose the existence and progress of phlogosis, and so take your choice of the whole armamentarium of the antiphlogistic measures that have been approved good during the long lapse of our Medical Histories and experiences.

444. I have shown you, with tolerable clearness, what has been thought, and what done, concerning the nature and treatment of our disorder from very remote times; and, indeed, it appears to me that, though two thousand five hundred years have passed away since we began to inquire into the methods by which we should hope to rescue our friends threatened with childbed fever, the entire catalogue of all our acquisitions has been fully and fairly made out by the younger Baudelocque in his work on the subject. What, then, is the treatment we have learned in twenty-five centuries to apply?

1. Bloodletting.
2. Emetics.
3. Purgatives.
4. Sudorifics.
5. Antiseptics, tonics particularly, camphor, and bark.
6. Blisters and other revulsives.
7. Cold douches and cold stupes, or hot stupes to the belly.
8. Sub carb. of potash.

9. Oil of turpentine.

10. Mercurials: he ought to have added opium.

445. Let us, in this Letter, inquire cursorily concerning the power of certain of the above-named remedies, to oppose the progress of intro-pelvic and abdominal inflammations.

446. Persons who, by accidental wounds or from diseases, lose large quantities of blood, are brought to a state of weakness, greater or less in proportion to the waste of the vital fluid they may have sustained; yet some, who have been excessively weakened by such hemorrhage, even after being apparently nigh to death, are found to suffer very little inconvenience beyond the temporary loss of strength, soon regaining all their activity and energy by the use of convenient food and drinks. There is no class of people who can be more familiar with the effects of hemorrhage on the bodily health than physicians in the extensive practice of midwifery; for they frequently meet with enormous wastings of the blood, in cases of abortion and of labors, whether before or after delivery, whose occurrences and incidents familiarize them with the effects of loss of blood. Many a time and oft have I stood aghast at the rapid effusions and almost exanimate condition of patients of mine in hemorrhage. Years of experience have, however, convinced me that as soon as the effusion is arrested the trouble is over, and no bar exists against the rapid and perfect recovery of even exuberant health. I have stood for hours near the bedside of a sick woman, applying all the resources of my art, and in great doubt, at times, whether the absent pulses could possibly return, or the pallor of impending death give place to the hues of health again. Well, these very people I have often observed to be in brilliant health at the end of the lying-in month. It is, I think, a great mistake to say that loss of blood, even enormous loss, is in any degree injurious to the constitution of the individual. The story told of Agrippina, who, by order of Nero being compelled to have her veins opened, was near dead when the tyrant countermanded the order and caused the wounds to be bound up, seems to me to have some influence on the general opinion concerning losses of blood. That lady is said to have been ever afterwards pale, having lost, on the occasion, the power of tinting her skin of a flesh color. But Agrippina's single case is

not fit to furnish an argument or proof for the many, since thousands of women in labor and abortion pass as near to the brink as the good princess could have done, and yet in three or six weeks are even in better health than they were ever before.

447. Practitioners of physic, even the busiest surgeons, can have no such familiarity with the effects of loss of blood, as the accoucheur; and this being the case, it is probable they are less timorous as to venesection as a remedy for inflammation, than most general practitioners and surgeons.

448. The use of the lancet as a remedy, seems to me to be subject to an influence of the nature of fashion; for there are cities and countries where venesection is much approved and made use of, and others where it is equally disapproved; and also, in the same places or countries, there occur series of years in which bloodletting is resorted to on all occasions, and other series in which no one seems to resort to it, except under extraordinary circumstances. When Fagon was Physician to Louis XIV., all the French doctors were the bloodiest practitioners in Europe.

449. But at the present day, a French physician, if he bleeds at all, scarcely goes beyond what he calls *une palette*, which is, I believe, about six ounces. Twenty years ago, in Philadelphia, the use of bloodletting was far more general than it is at the present day. We had then, and for many years previously, numerous individuals who earned a good living as bleeders, &c., but for many years past that class of people depend for their profits far more upon their leechings and cuppings, and most of them have added dentistry to the list of their avocations as means of profit.

450. Are we to attribute this difference in practice to the improved knowledge of our art, or to the influence of fashion? Will not the day return when the bleeders shall again be as busy as of yore? I doubt not the lancet will recover its vogue, and the sooner the better. Neither do I doubt of some slight influence of fashion and custom in this matter; but I attribute the change in our practice, as bleeders, neither to improved skill and knowledge, nor to fashion in chief, but to epidemic constitution, which causes us to resort, less habitually, to venesection as a remedy now, than we were wont to do a few years since. Such

changes in modes of practice must frequently coincide with the mutations in our epidemic constitutions, and as there will reign, for series of years, an atmospheric constitution to render venesection less needful and useful; so there must also arrive other series of years, during which the epidemic constitution of the air shall compel all discreet and wise physicians to oppose the progress and subdue the very existence of disease, by the promptest and most powerful of the antiphlogistic resources of our art: I mean bloodletting. Hence, when I listen to sneers on our forefathers for their bloody-mindedness, I seem to perceive, in a not distant future, the smile of scorn or pity for us of the present generation, who fear the click of a spring lancet as a struck fowl or a hurt wild duck, the report of a caliver.

451. What are you going to do to save the woman who, having been put to bed the day before yesterday, you find now attacked with rigors, intolerable pain and soreness of the lower belly; violent fever and headache, and pulses at 140? Come, young gentlemen, let us see what it is that you will do.

452. In the first place, I presume you will go about to settle the question of the diagnosis, and if your examination should result in a conviction that it is a childbed fever that has fallen on the patient, you must admit that she has inflammation, that began somewhere within the pelvis; that is, either in the womb, in its veins, or in the peritoneum; and upon this conviction is superimposed the grave reflection, that this inflammation may be justly deemed to carry in its own nature no dispositions whatsoever towards a cure of the patient, the termination of them being almost invariably, either a suppuration, or effusion with adhesions, and occasionally gangrene; results, either of them, that must prove mortal to the sufferer.

453. If you should agree with me, then, that childbed fever is an inflammation, and not a fever, and also allow that such inflammation, running its course to a term, destroys at that term, then assuredly you will resolve to do something that may probably check further expansion of its area, and haply impress upon it a tendency to recover by resolution. And what shall that thing be? shall it be an emetic? an emetic to cure a phlebitis or metro-peritonitis!

454. Hem! shall you bleed her? Very well. Shall you give her calomel, to the same wise end? Some turpentine; some Jesuits' bark; some camphor; some opium; some kermes; some tartar-emetic; a blister; some leeches; a cupping of the hypogastric region; or a poultice on the whole belly? Will any one, or all of these medicines together check the progress of that phlebitis, or peritonitis, that is to drag her down to the grave, provided it is not cured, and promptly cured?

455. It matters little what shall be the precise and definite result of your diagnostical researches, as whether you conclude the case is purely peritonitic, or whether it is metritis with the well-known frequent complications; for, whether it be the one or the other, or all of them combined, you must, after all, rely upon the power of one or more of the above-named remedies. The great matter is, to begin soon and to act strongly and decisively. Whatsoever you find to do, I pray you do it with your might; for as there is little time to work, the loss of a few minutes may be irreparable. Have I not mentioned an instance in which death ended the scene in eight short hours after the commencement of the disorder? but if such be the precipitancy with which the areas may *possibly* expand, the necessity of beginning early cannot be questioned. *Obsta principiis* is a good motto for all who are to treat such disorders as these.

456. I find that I am now come to the most difficult part of my task in this work, that of pointing out a proper and hopeful method of treatment. I am but too well aware that there must occur many cases in which no treatment can be of avail to save the life of the patients. It is the same with cholera and yellow fever—diseases that demand, and must have their quota for the grave. This I freely admit; yet it is a source of the highest consolation, that of believing that one acts, not only with conscientiousness, but with wisdom and great prudence.

457. In selecting the proper remedies, and determining the proper times for their application, the physician best displays his power. I have given you a general catalogue of those remedies, and I shall in my next Letter discuss *seriatim* the question relative to the choice and way of exhibition of them; but now I rest, with respectful regard.

C. D. M.

LETTER XIX.

ON THE TREATMENT BY MEANS OF BLOODLETTING, AS THE REMEDY—IN-CHIEF FOR CHILDBED FEVERS—ESTIMATE OF THE QUANTITY OF BLOOD MOVED BY THE HEART IN GIVEN TIMES, IN HEALTH AND IN DISEASES—THE POWER OF BLOODLETTING AS A CONTROLLER OF THE CIRCULATION, AND CONSEQUENTLY, OF THE INNERVATIONS—ANODYNE POWER OF VENESECTION—INSTANCES IN POINT FROM ARMSTRONG, FROM GORDON, FROM LEGOUAIS, FROM AUTHOR—PUZOS—DR. LEE'S TABLES OF CASES TREATED WITH AND WITHOUT VENESECTION—GORDON—RULE AS TO TIME—HOW TO BLEED—GORDON'S PUPILS—LATE VENESECTION—A FATAL CASE—TWO CASES SUDDENLY CURED—A SUDORIFIC MIXTURE—CASE OF VENESECTION AFTER VIOLENT HEMORRHAGE.

458. MY purpose in writing this letter is, to lay before you my thoughts on the subject of bloodletting as a chief remedy in the conduct of those alarming disorders we have been discussing in the previous pages of this work. In doing this, I hope I shall be able to point out, with precision, those forms and stages of our disease, in which you could scarcely err in adopting the use of venesection, as well as those more questionable cases in which an experienced physician might hesitate; and also such as are clearly gone beyond the bounds within which bleeding is curative, and outside of which it would prove dangerous, or perhaps fatal. Unhappily, it must hereafter, too often happen for you to be called in to persons, on whom the hand of death has already been laid, long before your arrival; leaving you only to act as reluctant, but sympathizing spectators of a ruin for which Providence has ordained no remedy, and for

which you could scarcely find, in all the resources of your Art, even some slight alleviations.

459. The opinions I shall put forth in this Letter, are the same as those I have held for a great number of years; that have governed my conduct as practitioner in the cases; and which in various publications, and in public lectures, I have maintained, in opposition to many writers, and many friends and acquaintances, who prefer other modes of treatment than mine. The criticisms with which I have been honored in medical journals—some of them, it is true, very rude ones—have not had the least influence to change the convictions forced on me by reflection and clinical observation; nor shall I, on account of such disagreements among my brethren, living and dead, refrain from giving you my whole thoughts. I am not one of those persons who deem their sentiments upon a point in Science or Art, as of kin to their morals; and who feel insulted when the whole world do not consent to and admire their own particular way of thinking. Indeed, it is to me, in general, a matter of indifference what men do think on questions of science or rules of art; though, I confess, I sometimes feel sorry for those sick people who fall under the guidance of incompetently educated doctors. My own views as to the indispensable necessity for bloodletting in childbed fevers, can never be abandoned because there are multitudes of people, in and out of the profession, who look upon a venesection as a great and important thing, that ought never to be undertaken, except under the most urgent necessity. Why, indeed, should I withhold from you my sincere opinions because of the class of people just referred to; since there are, on the other hand, vast numbers of the brethren, as well of those who died centuries ago as of living physicians, who go with me in regarding this as the chief sanative means delivered down to us from ancient times? I know not how it is that I have, by some critics, been accused of possessing a presumptuous, dogmatical style of thinking and writing, which has proved offensive to them; for I do not feel at all disposed to cram my opinions down the gentlemen's throats; I merely present them for acceptance or rejection, at their own good pleasure. To me it is nothing whether they be received or spurned, for I cannot make them true if they be false, nor can any man convert them into a lie if they be really true. Truth is

no man's. Truth is God's. Wherefore, then, should any reader of mine become incensed on account of the warm zeal with which I design to set forth what I deem to be the most important—most salutary truth! During my tedious studies of the subject on which I am now writing to my Class, I have observed the most irreconcilable differences of opinion: it might be considered almost correct to say, that these differences are national or even ethnological; for while, in one country, and among one people, the lancet is freely and boldly employed, in others it is taken up with a seeming terror of its power. In Germany, from the time of F. Hoffmann, who bled in the foot (vol. ii. p. 158), and in France, from the time of Fagon and Puzos, down to the present day, the former, with some rare exceptions, are not bold bleeders, while the latter are bleeders *coup sur coup*, as old Puzos expresses it. These differences are among the most surprising things in Medicine; and as their existence proves the utter incompetency of nature and observation, at least hitherto, to bring us all into one line of opinions, they appear to me to be disgraceful to physicians as a learned class. I conceive that these differences clearly prove that, as a class, we learn more by tradition than by observation and reflection; for, as well might one present a bright daguerreotype plate to a scene, and expect it to give us the representation of a different scene, as to offer the human understanding as a *vasa tabula* to the facts before it, and look for so many different interpretations as we find in the most simple questions, *e. g.* this of venesection in childbed fevers. I say, therefore, that if we do not agree, it is not because nature does not agree with herself, but because we take our notions traditionally, and not from careful observation. But, whether we have learned by relation or by inspection, we cannot help thinking what we do think; and I hold myself, therefore, free to say that, inasmuch as I consider all treatment of childbed fever both futile and ridiculous, when based on any other views than that of controlling the blood's force, both in its substance and its motion, by means of the lancet, I shall as warmly and as zealously as I can, strive to convince you that bloodletting is a convenient, safe, effective, and most indispensable means, and constitutes by far the chiefest, if not sole resource of our Art, in those dreadful disorders.

460. Let no man, therefore, I pray, take offence at a Medical

brother for his Medical opinions; nor let me be charged with dogmatism and presumption for maintaining opinions that have struggled for the mastery these two thousand years, during which they have actuated many of our best men, in practice, in various countries, as they do now. I wish you, my Students, to believe that my heart's desire and my hope is, that you should learn the truth as it is, and not my opinions. What are my opinions to you, or to any reader of these Letters? I shall very soon be numbered with the forgotten dead, and as indifferent to you then, as you will then be to me. It is not my opinion, then, that it interests you to know; but, rather, the whole idea of the disease and its cure that is to be formed in your minds, by your own direct observation and reflections upon facts observed. Do I state facts in this Letter? Then let them have the force of facts. Do I set forth opinions, hypotheses, conjectures? Then let them have only the force of surmises or guesses at truth. It is yours always to judge of the truth of facts, and the value of reflections presented by any writer whose work it may please you to examine. I here again assure you, I am heartily ashamed of the scandalous discrepancies, and the pitiful insufficiencies I find in our library books upon the subject of childbed fever, and that I fervently wish it were in my power to set the whole truth of the matter so clearly before every one of you, my Students, that you should, each and every one, be able to perceive and know the Truth on a subject, so important to society, and to us, as a class of the learned. To use words of Quintilian, I could wish, in writing these Letters: "*Ut ipsæ sermonem finxisse Gratiae videantur, et, quod de Pericle Veteris comœdiæ testimonium est, in hunc transferre justissime possit, in labris ejus (mine) sedisse quandam persuadendi Deam.*" (*Inst. Orat.* x. p. 81.) Alas! for me; no such testimony will be accorded to my speech, nor such Goddess, sweet *Sua da*, inspire my pages!!

461. As a physician, in practice, I could not but be much given to confide in phlebotomy as a means of conservation against threatened attacks, as well as of cure for sickness already formed; forasmuch as in all my views of vital action, whether physiological or pathological, I have never been able to conceive of either sense or motion as things possible, independently of a quantitative state of the circulating blood. I speak here of

the capillary blood, in the part wherein the sensation is perceived, or the motion effected. I think I shall never become able to view this matter in any other light; for I am convinced that all acting power of the nervous mass is an absolute direct consequence of the combination of oxygen, in the blood, with the albuminous element of neurine. It is only by this combination with oxygen, that the nervous mass can evolve or extricate and send forth along the nerve-tracks, the nervous force or neurosity—or, as a Frenchman, M. Cerise, writes the word, *nevro-sité*. Febrile diseases, and all inflammations, are, manifestly, conditions or states growing out of over-production of this neurosity, in the whole or in the parts. In the idiopathic fevers, the nerve-centres are the parts first affected and modified by quantitative states of the blood, as well as by its qualities; and the constitutional perversions flow out of those quantitative and qualitative states.—The oxygen taken up by the blood in the pulmonary capillaries, except the capillary branches of a branchial artery, is given off again in the systemic capillaries, which are thus a sort of strainers, in which the oxygen is separated and detained for the uses of the organs and tissues. The brain capillaries give out the oxygen, whose combination with neurine, elicits the flash of life as the biotic force or nervous force: hence, quantitative states of blood in the encephalon, determine the quality or force of the nerve stream. In inflammations, on the contrary, it is the organs or tissues that are first to be affected; and from such affections or modifications of their vital state, flow the constitutional disorders, reactions, or perversions. The former are centro-peripheral; the latter are periphero-centric maladies. They are diseases of opposite poles; the idiopathic fevers being affections dependent on states of the encephalic pole, while inflammatory diseases and disorders of the organs are results of failure or derangement in the organ-poles of the body.

462. Tissues in a state of inflammation may be said to tend always to expand or augment the areas of the phlegmasia, in a ratio, compounded of the force of the blood's motion within their capillary vessels, and the vital power of vascular resistance to the injecting force of the heart. A strong, full, frequent pulse, tends more to the expanding of the inflamed areas, than a weak, small,

slow one. Therefore, to diminish the injection-power of the heart, is to lessen the tendency to expansion in phlogosed areas; and this truth is daily seen in clinical practice, where we constantly observe that inflammations do speedily terminate by resolution, after bloodletting, which, but for the curative power of that remedy, would certainly seek and find their term in effusion, in suppuration, or in gangrene.

463. A patient having a strong, large, and frequent pulse, with cephalalgia, or pleuritic stitches, or a peritonitic pain, immediately acknowledges the relief that ensues upon a free venesection; the headache lessening at once, the pleuritic pain vanishing, and the anguish of the peritonitis giving place to a freedom from pain, and tolerance of pressure, before insupportable. The senses are become freer, the respiration is more natural, and heat and fever, and indeed all the excitement and turbulency of the life are reduced, by the venesection, to states of a comparative calm physical complacency. But not only does a proper bloodletting procure such relief, it is also true that it is frequently followed by a prompt cure of the whole disorder; for, it many times happens that an inflammatory disease, such as childbed fever, which has made regular progress from bad to worse, up to the moment of the operation, does from that moment wholly change its rate, and as steadily change from worse to better, until no vestiges of the disorder are left behind. The disease, even a dangerous and advanced one, is not unfrequently cured by a single bleeding. I think I have described such a case in sections 467, 493, 497, 501, and 502, to which I beg to refer you; and our books are filled with examples which cannot be gainsayed. Dr. Armstrong has, in particular, the following one at p. 200: Mrs. R., a stout woman, æt. 28, delivered June 2d, had, on the 4th, rigors, headache, nausea, and retchings. Ten hours after this attack, Dr. Armstrong found her with pulse 134; skin pungently hot, tongue white and dry; milk and lochia nearly suppressed; dysury; quickened breath; belly so tense and sore as not to bear the weight of the clothing; countenance anxious. She complained of oppression, pain, and weakness. She was bled 24 ounces, and took 20 grains of calomel, followed by sulphate of magnesia; drank barley-water with isinglass. At 6 P.M. was bled again, 12 ounces, and took 20 other grains of calomel with

sulphate of magnesia. On June 5th, 6 A. M., the blood drawn last evening was found to be much buffed; the patient had been freely purged, and the belly was now soft, and bore pressure well. Pulse 100; skin moist and warm. Took an anodyne enema, and slept several hours, and from that time might be considered convalescent.

464. Now, if you will pause a moment and reflect upon the condition and prospects of that woman, when Dr. Armstrong was called in to cure her; upon the then existing state of the inflamed tissues within her pelvis; the convulsive movement of her blood; the exceeding derangement in the innervative operations of her nervous mass; it seems to me, you cannot well deny that the physician did save his fellow-creature's life; and that her rescue was due, in chief, to the venesections; for, assuredly, you will not permit yourselves to think the calomel, the salts, and the anodyne enema could have so rapidly appeased her greatly disturbed constitution, and so quickly brought her into a state of convalescence.

465. Legouais's *Reflexions et Observations sur l'emploi de la saignée et des purgatifs dans le traitement de la Péritonite Puerpérale*, at p. 66, gives us the case of a certain Augustine Lef * * *, æt. 36, a seamstress, at the hospital called La Maternité, at Paris. She was delivered of a daughter at 4 o'clock P. M., August 17, 1818, by a 24 hours' labor, in which she had been bled for rigidity of the cervix. May 18, she had a mental shock, and immediately after it was seized with a chill that lasted an hour and a half, followed by pain in the lower belly, soreness of the right hypochondrium, and difficulty of breathing. At 10 P. M. the belly tender and painful over almost the entire surface; very hot and moist skin; pulse hard, full, and very frequent; face flushed and excited; venesection 22 ounces; a purgative dose; diluent drinks; cataplasm to belly. Four hours subsequent to the venesection she was much better; the pain and general symptoms continued only in the slightest degree. She was better all night, and, on the 19th August, in the morning (second day), almost no pain, though the pulse was still somewhat accelerated, and skin warm. Aug. 20 (third day), the pulse still a little frequent, and skin warm—she was much better; on the 25th of the month she went out cured,

being only a little weak and pale. Such is M. Legouais's account, and I ask you to pause again for a moment to inquire whether this woman's recovery, as well as Elspet Robertson's, below, would probably have taken place, if venesection had been omitted. I think not.

466. As cases of this sort are highly illustrative, as well as instructive, I shall beg you to take one from Alexander Gordon, p. 82, who says: "I was called to Elspet Robertson, No. 58, a few hours after the attack of the puerperal fever, which took place on the day after the delivery. The patient complained of a very acute pain in the abdomen, which had succeeded a severe shivering fit, and the pulse was at the rate of 160. She was bled to the extent of 24 ounces, and got a purgative at 2 o'clock in the afternoon, immediately after the bleeding, which produced six or seven plentiful motions; and, when I saw her at eight o'clock in the evening, to my great surprise, the pulse had come down from 160 to 108, and the pain of the abdomen was gone. Next morning, when I called, I found her without fever, pain, or any other complaint." Mrs. Thompson's case, which he also relates, gave precisely similar results, as did also Mrs. Forbes's, which I intend to quote for you in another page.

467. Here is a case from my own note-book. Saturday, Feb. 26, 1853, Mary Anne Alli * * *, primipara, æt. 29, after severe labor for sixteen hours gave birth to a healthy boy, at 2 o'clock P. M. She continued quite well until 12 o'clock P. M. of Monday, the 28th, which is fifty hours. She was now seized with severe chill, and intro-pelvic and hypogastric pain. I did not visit her until 12 o'clock on Tuesday, 29th, when I found her already enormously tympanitic, and in violent abdominal pain; red face; hot skin, and pulse 130; belly sore to the touch. I concluded she was attacked with metro-peritonitis, of which I am not now in any doubt. I bled her, and measured in the bowl thirty-one fluidounces, that I took from a large orifice in the arm. I gave this woman no calomel; she had cataplasms of Indian mush to the belly, and took Chaussier's aperient mixture of castor-oil and syrup of rhubarb. She also got several aperient enemata. From the time I bled her, I never felt any anxiety about her safety, and, in fact, she got rapidly well, being

cured by the good bleeding that I gave her when her areas of phlogosis were already twelve hours old. It would have been better to have bled her at 4 A. M. or 6 A. M. of the 29th. On Thursday, March the 9th, I found her sitting up, with the child at the breast, quite in good health.

468. I again pray you consider the above relation, and judge whether this woman would have lived, had I made an attempt to cure her without employing that fortunate bleeding. Consider, also, that I gave no mercury, and administered only a very simple and gentle aperient, with enemata. You, the least among you, would have cured her in like manner.

469. These veucsections did no injury to the women, or to others that I have treated in like manner; on the contrary, they were, under Providence, the means employed to save their lives; and I consider the use of bloodletting to be peculiarly appropriate to the congestive and inflammatory disorders of lying-in women, whose nature and want it is, in and after labor, to discharge considerable quantities of their blood. While it is true, that I have a thousand times been startled on witnessing the immense effusions that have occurred in the course of my obstetric experience, it is also true to say I have been equally surprised at the little inconvenience suffered from uterine hemorrhages, after the cessation of the flowing. And this I present to you as an argument to be addressed to your own reason, whenever you shall stand hesitating and doubting as to your duty, in the treatment of the inflammatory or congestive disorders of your lying-in patients. As a general rule, it does no harm to such a woman to bleed her very abundantly. Among the older writers who had the good sense to perceive this truth, was Mons. Deleurye, *Traité des Accouchemens en faveur des Elèves*, who, at p. 471, says: "The public, as well as some physicians, still think that bleeding at the arm is not suitable in these cases. This is an error, and there is no better remedy to be found in the early stages. They should be repeated every three hours, and the first venesection should be copious, without paying any attention to the vaginal discharges, and being persuaded to adopt the principle that this disorder is to be treated as a very decisive inflammation, that soon runs into gangrene."

470. Having in a former page, expressed my admiration of the signal ability of the celebrated French physician Puzos, I wish here to cite a passage, in which he urges the use of the best of the antiphlogistic measures, in the treatment, even, of questionable cases. I mean cases where, though there is hypogastric pain and fever, and persisting lochia, the diagnostic may not be clearly established. As an answer to these objections, he considers that we ought to reflect that a smart fever, attacking a woman within a few days after her labor, should be compared to a pleurisy, a defluxion on the breast, an inflammation of the liver, or a violent fever with strong determination of blood to the head. Now if it should happen in any such case for the patient to be seized with Hæmoptæ, or epistaxis, or hemorrhoidal hemorrhage amounting to five or six ounces in twenty-four hours, do we rest satisfied with such an evacuation? Do we not rather take away two or three pints of blood, and sometimes more in the same space of time? p. 369.

In a subsequent paragraph, after admitting that he had seen some persons recover without venesection, and by means of abundant bilious discharges or sweats, &c., he confesses that he takes no credit to himself, or his method, for such results, as they are attributable, rather to the powers of nature, than to the art of the physician. De La Roche, *op. cit.* p. 158 says: "I believe that in almost all grave attacks, it (venesection) is the essential remedy, and that on which we must principally depend for the cure. It must be used to prevent suppuration and gangrene in the abdomen, just as we employ it to prevent empyema in pleurisy." He insists upon an early recourse to it. The longer we put it off the worse for the woman; for if the disease is well established it cannot be cured without larger bloodlettings, which are the more dangerous the longer they are postponed. What a strange thing it is that the French and English practitioners should mostly rely upon the vigorous use of bloodletting, while the whole mass of the German physicians appear most reluctant to employ the lancet, depending upon drugs and medicines to produce effects on the living economy, that the Author of nature never did vouchsafe to those articles. If a German should bleed a woman with her peritoneum on fire with inflammation, he would scarcely venture to abstract more than eight or twelve ounces,

lest he should interfere with the lochia or the milk. What is the use of lochia or milk for a woman hastening to end her childbed fever inflammation by its most ready term; I mean effusion, suppuration, or gangrene?

471. Dr. Robert Lee, of London, in *Lectures on the Theory and Practice of Midwifery*, &c., at p. 457, presents a table of 160 cases of severe inflammation of the uterus and its appendages; with notes of the symptoms, treatment, and results. Of the 160 women, 72 died and 88 recovered. Some of them were treated by venesection, and others were not bled at the arm at all; certain, only, of them having been subjected to leeching. Of the 72 who died, 32 were bled and 42 were not bled; but of the 88 who recovered, 70 were bled and the remaining 18 treated with or without leeching, so that, out of the whole number of 160 cases, 102 were bled and 68 not bled, except with leeches for some, while for the others, no sanguine depletion was prescribed. It ought to be observed that, in a good many of the above 160 women, nothing was attempted, in consequence of their perishing state when first seen by Dr. Lee.

472. It appears to me that any person well-informed concerning the inflammatory lesions as to childbed fever, and acquainted with its indefeasible tendency towards a termination in effusion, suppuration, or gangrene, should agree that the happy recovery of 88 of the 160 women was, in a good measure, due to the venesections, which were doubtless reinforced by judicious after-treatment. The table does show, in the clearest manner, that the use of the lancet is not so hazardous as many suppose it to be, and that used as Dr. Lee employed it, with boldness, it not only does no harm, but many more women recover under its exhibition than in any other way.

473. There is much room for congratulating Dr. Lee on his rescuing so many women, attacked with this dangerous disorder; and it is to be reasonably supposed that though venesection did not have power to save the life in 32 of the cases in which it was employed, while it was not prescribed in 40 of the 160 women, no person should, from this want of success in the 32 cases, go about to decry bloodletting, because, if in 40 cases it was not ordered at all, there must have been notable good reasons for

the omission, in the hopeless condition of the women, when brought under notice; as well as a very strong inference to the violent or advanced stage of the attack in those who were bled, but without fortunate results. It is not likely that Dr. Lee would order blood-letting for a woman to whom he should be called when she was already in a dying state; and it is equally probable that he would give the patient the best chance of a cure, by bleeding her, even where there might be some doubt as to the success of the operation.

474. I confess I cannot but take great encouragement from Dr. Lee's table, as to the efficacy of the treatment by venesection. You ought here to observe that, in many of the cases enumerated in the table, Dr. Lee did not himself direct the treatment, and in many of them the dissolution was imminent previous to the exhibition of any remedies, and that such cases present no indications of the use of the lancet. You will learn, presently, that the disorder often pursues a course so rapid that, for many women, the curable stages are already past, before the attention of the physician is called to them, as was many times observed in Dr. Collins's cases at Dublin Hospital. Having shown, by the foregoing histories, that these inflammations do yield to venesection, and that it is a safe and convenient, if not indispensable, means of cure, let us now return to a more direct consideration of the power and effects of bloodletting in these childbed fever inflammations: and here I invite you again to notice the state of a circulation in which the contractions of the heart are repeated at the rate of 130 beats a minute. I say 130, though it is very common to count 150, or even 160 pulsations to the minute. Each systole of the heart expels from the systemic ventricles, perhaps an ounce, possibly only half an ounce of blood into the aortic trunk. When the pulse is full and strong, it is to be presumed that a larger injection is driven into the artery with each beat of the heart. If you assume one ounce of blood as the quantity expelled from the ventricle at each pulsation, then you find rather more than nine pounds avoirdupois of arterial blood is injected into the aorta, to be distributed throughout its branches, in every consecutive minute, with pulse at $150=540$ pounds per hour. I have no doubt that in some cases, when the synochus-fortis pulse is observed, particularly in the first hours after the attack, this computation is far from excessive; but, if

you please to reduce it to only the half of the estimated quantity, you must be struck with the enormous effort by which the transfer of nine pounds of blood per minute, or 270 per hour, in its circuit, is effected; and how dangerous must be so impetuous a torrent, driven into the inflamed capillaries of a metro-phlebitic patient, or an extensive and extending peritonitis in a lying-in woman!

475. It is to oppose this morbid vital state, that you attend at the bedside, with remedies in your hands; and very certain it is, that, seeing the nature of the lesions, and reviewing the corporeal condition of the woman, just escaped from the fatigues of pregnancy and labor, and about to undergo those essential, indispensable, involutive processes that alone can reduce her to a natural non-gravid condition again, you will make a fatal mistake if you begin by a wrong or futile way of treatment. What will you do? You may give her an emetic that will cause vomiting; but what will it do more? You may dose her with Dr. Brenan's oil of turpentine, yet with what influence on all this perturbation of her brain, heart, and lungs? You may dose her with quinia, but what is your hope, and prospects? You may crush or overwhelm her sensibility under your anæsthetic doses of opium. But can all these surely change the disordered relations betwixt the intro-pelvic and abdominal tissues and the nerve-centres, that are extricating, as if by a sort of electrical diruption, or explosively, the neurosity produced by the combination of oxygenated blood with the neurine? Will you try it with calomel? Why, some women have taken 900 grains, and died! Calomel! Look again at my sect. 322, for Kiwisch's patient, who died in eight hours, and multitudes like it in the books everywhere. Calomel! How long will it be before the calomel shall begin to control and moderate this furious torrent of the circulation! There is not time for calomel. Calomel cannot compose this passion. It is of education Ovid speaks when he says, "*emollit mores, nec sinit esse feros*," not of calomel. There is no time for the action of calomel; the disorder is swifter than the winds, and your calomel moves with the reptant pace of the tortoise; don't depend on the calomel; these are not the cases for drugs alone.

476. I solemnly aver that, after almost half a century of medical studies, and most extensive and copious practice, I do not know

a situation in which a medical man can be placed, wherein he should require a more perfect intimate knowledge of the powers of remedies, or be able to estimate their differential value, with keener perception of even the shades of difference between their several worth, than this of a childbed-fever-inflammation just begun. It is a time to look upwards and pray to God for both judgment and courage; to reflect upon the heavy responsibility, and then take it, with an upright heart, and a mind resolved. I am not speaking to you as my children and pupils, without earnest thought, nor unadvisedly; for I strongly remember, not only my own experiences, but Gordon's distress when pursued by the fatal epidemic at Aberdeen. Gordon was, at the time, most sorely beset. He knew that the lives committed to the protection of his ministry depended upon the right employment of his great antiphlogistic; yet his sentiments, or heart, revolted against his judgment, whenever he beheld the appalling weakness, or looked upon the typhoidal exhaustion that would scare him from his purpose. What says he? "Now, nothing can be more applicable to the puerperal fever than the observations of Botallus; for, when I took away only ten or twelve ounces of blood from my patient, she always died; but when I had courage to take away twenty or twenty-four ounces, at one bleeding, in the beginning of the disease, the patient never failed to recover;" *op. cit.* 78. Again, at p. 79, "unless a practitioner venture to take away the quantity mentioned," &c. "And I felt this prejudice in its full force when I had not courage to take more than twelve or fourteen or even sixteen ounces, &c. &c. But when I had resolution to take twenty or twenty-four ounces at one bleeding, I disregarded it, because I was sure that that quantity, taken away within six or eight hours after the attack, would certainly cure the disease," &c. &c.

477. Though I have examined with greater or less diligence all the treatises, very numerous, on these disorders, that have come within my reach, yet I cannot but consider the above words of Alexander Gordon as worthy, more than any others, to be printed in letters of gold, and I consider the granting of them to mankind as a great and useful bequest to human society. Any man who will read and comprehend them, and apply them according to their real meaning and intent, is fit to be entrusted with such

important interests as these; and there can be none greater in the entire range of the physician's ministry. Gordon speaks of having courage to do the bidding of his judgment, and of resolution to carry out the dictates of his conscience, unbiased by minor considerations, and unterrified by appearances, which, however well calculated to deter some men, could not make him flinch from the exercise of his power, under convictions of duty.

478. I have represented you as attending at the bedside with remedies in your hands, and interrogating your judgment as to the selection of the proper ones.

If the disease should not have already proceeded so far as to render it clearly impossible to prevent the termination in either effusion, adhesions, suppuration, or gangrene; then, I conjure you, that, laying aside all other remedies as futile, you resort to venesection as the sole hopeful resource in the case. But if, judging from the appearances, you cannot indulge the hope of preventing those internal effusions and suppurations, you should consider that the time for the application is quite past, and that you would, by administering it, only hurry on the fate of the victim.

All that I have above said ought, I think, to convince you that venesection has been in numerous instances used as a remedy in childbed fevers with a striking success; and though I have not had the happiness to show you all the *minutiae* of the cases in which it was employed, the naked fact stands out before you, that sick women recovered speedily, conveniently, and perfectly; and this might suffice to show that it is not only an allowable, but a most reasonable and hopeful remedy. You ought, of yourselves, to settle the question whether the use of venesection does or does not moderate the force of the circulation, and whether that moderating influence ought, *a priori*, to be looked upon as the best adapted of all our means to promote or favor a cure by resolution. Compare the mollifying influence of a proper bleeding with those of calomel, turpentine, emetics, or opium; take notice of the length of time occupied by each in bringing about the good results expected from them, and see for yourselves whether you will venture to wait, even an hour, to procure a benefit by one, which you can secure in five minutes or less, by the other. I

hope these remarks may lead you to conclude that the greatest vigilance, as well as promptitude, should be always observed, since the loss of a few moments, in so rapid a disease, might compel you to change your entire plan. You must be present very early, for there is an hour in the case, subsequent to which you neither can do nor even hope to do any real service.

479. I do not, however, conceive that any person has a right to fix, beforehand, the precise date under which venesection may be or may not be prudently employed, because the rate of the disorder varies greatly in different women, some of whom complete the whole of the inflammatory processes and die, within 20 to 40 hours, while others maintain a doubtful conflict during 5, 10, or even 15 consecutive days. Gordon, *op. cit.* 74, says: Two died in the space of 24 hours after the attack; one in 36 hours; three on the 3d day of the disease; fifteen on the 5th day; three on the 7th; three on the 11th; and one on the 23d. Those of our brethren who most distrust the lancet as a remedy, agree with the warmest advocates in recommending an early resort to it, if any. Gordon, 91, says: "After much experience in the disease, and mature deliberation concerning the conduct most proper to be pursued in my peculiar situation, I came to the following resolution: if called to a case within twelve hours after the attack, I insisted on bleeding the patient, and promised for its success; but if at a later period, viz: from twelve to twenty-four hours after the attack; in that case, like Sydenham, with the same remedy in the smallpox, I thought it incumbent on me to propose it as the only effectual remedy; but I neither insisted on it, nor promised for its success."

480. I consider that the extract from Gordon that preceded this one ought to weigh more than his assertion as to the lapse of precisely 12 hours; for it is certain that if, in the two women who died in 24 hours, and in the three who died on the 11th day, there had arisen a question as to venesection as a remedy, it would be far more urgent to adopt it very early in the first two, than in the latter three women. Nevertheless, I am well satisfied with Gordon's preciseness, which serves, at least, to prompt us to be more vigilant and active, and so, more fit depositories of the public confidence.

481. I do not think it worth while here to attempt to point out to you the precise and distinctly marked signs out of which you are to gather the indications, *pro* or *con*, as to the use of venesection in your cases. I have observed that, for twenty-five centuries past, this could not or has not been done, at least with the effect of making the matter clear to all minds. Yet these signs do exist, and are susceptible of a rational interpretation. They are not to be discovered in books, but they are abounding and superabounding in nature. In the difficult cases, a man cannot master them by studying the pulse alone; indeed, it would sometimes be safer not to feel the pulse at all, as the pulse is capable of entirely misleading us. Neither shall you know the truth by means of auscultation of the heart, to determine its rhythm and force, for they may mislead like the radial pulses; nor by any spirometric estimates of the respiration, its amount, its rate, and its efficiency in oxygenating the blood and the tissues, and in evolving the animal heat; neither the muscular strength, that often fails, as an expositor of the vital forces; nor the intellection as exhibited in speech, in reasoning, or physiognomical expression. It is rather from an examination of the whole of these, than from a part only of them, that a man may come to his conclusions. Time, or the past duration of the case, is an important element, that should always be taken into the account with our decision. To touch the pulse alone, therefore, and find it flying; to observe the artery as being soft, compressible, and fluctuating, and at the same moment to note the appalling, and, so to speak, mortal pallor of the countenance; the hurried and broken respirations; and to hear the trembling voice; and see the unsteady tongue, might lead one to infer that the time of rescue is past, the disease gone too far, and the chiefest of all our resources barred out forever in the case. It is better, therefore, to pause and consider the conflicting elements of a diagnosis and prognosis; to reflect that the nervous centres are suffering from a state analogous to what surgeons call shock, which here leads to, and gives hope for, no reaction like that which may be expected in surgical shock, but is progressive to an issue which is only in suppuration, effusion, or gangrene—death. Dr. Hulme, *Treatise on Puerperal Fever*, Syd. Soc. edition, p. 76, says: "Nurses and women in general seem, in a great measure, ignorant of such a disease as this being incident to lying-in persons. I dare venture

to say that the very name of it is as much a stranger to most of them as if no such malady existed; and yet there never was a time when this disease did not exist. The consequence is, that, knowing no danger, they fear none; whereas, on the contrary, they should be taught to dread the name of puerperal fever as they would the name of pestilence or plague, for I fear the one destroys not more than the other. Like a fierce and untamed enemy, the one spreads his hostile banners in open day, and feasts on carnage and destruction till, glutted with slaughter, he himself sinks down and dies. But the other, like a secret, revengeful foe, stabs in the dark to the very vitals, and, though he kills one only at a time, yet he is privately slaying every day, and is never satiated; thus making up by length of time what the other does by a sudden devastation. They (nurses) should be taught to know, I say, that pain and soreness of the belly, coming on soon after delivery, unless speedily relieved by judicious assistance, will prove mortal in a few days. * * * I cannot say how it may be with other physicians, but, for my part, I never hear a childbed woman complain of a pain and tenderness of the abdomen, but I look upon her disorder with as much anxiety and circumspection as if I knew her actually laboring under an inflammation of the bowels, or a hernia with a strangulated gut; and I think it my duty to be as expeditious in relieving the one complaint as the other." What a pity it is that every woman who is about to be confined should not have before her the privilege of reading the above clear, strong, and most true words of Dr. Hulme.

482. How, then, young gentlemen, shall we judge? Who shall be able to teach us our duty? Such, it seems to me, are your questions. I, for one, cannot teach you, nor, I believe, no man else. You must learn it yourselves, out of the ample page of clinical observation, and by the exertion of your faculty of reason and judgment; not as empirically, but rationally, founding yourselves upon the sciences of anatomy and physiology. You alone must ever judge of the case and its wants, in the presence of the case. It does not require a long series of years to learn this; but it can only be acquired by the diligent, conscientious student of our Literature and of Nature, who must be from the beginning prepared to observe, and to know the truth of what he observes.

483. I have reflected long and anxiously upon Gordon's words.

relative to early venesection, and the absolute dependence to be placed upon it as a remedy, when promptly, resolutely, and courageously executed. Those words of Gordon seem to have been uttered by him as in a moment of enthusiasm and elevation that carried him beyond his ordinary and phlegmatic modes of mind. These reflections have led me to imagine that a physician placed under such circumstances ought always to be vividly on the watch, lest he should be blinded and misled as to the appearances and the facts before him. Such a man should possess an intellectual power capable of lifting him, as it were, to some great advanced height, from which to survey and overlook the entire scene, and discern all, not here and there parts of it only. He should be able, while clearly discerning the prominent objects beneath him, as the circulation, the respiration, the heat, the coloration, and the general innervation of the economy, to conceive of and comprehend the real condition of the textures, their vital state and reacting power, and so, become the prognosticator of issues and events wholly hidden from the eyes of the more casual and less eminently stationed observer. He would look into the actual condition, phase, and progression of the phlegmasia, and its tendencies as to the whole economy of the life. This is the monster cause of all the mischief, hidden in mists and fog, that he is to seek for and destroy at one blow. Thus it is that Gordon would instruct him to do, that the victim may be saved, and the Divine Art triumph in the rescue. A physician thus placed, and surveying the whole field from some great intellectual elevation, and scrutinizing each particular, bearing upon his duty and power, would be like unto the studious orator, supposed by Quintilian, in the tenth book: "*Tum intendendus animus, non in aliquam rem unam, sed in plures simul continuas; ut, si per aliquam rectam viam mittemus oculos, simul omnia quæ sunt in ea, circaque intuemur, non ultimum tantum videmus, sed usque ad ultimum.*" A man capable of acting thus will never be scared away from the performance of his duty by the outward aspect of the sick woman, her apparent exhaustion or debility; but, as Quintilian says, he will see not only to the end, but everything intervening betwixt the present and that end—in plain words, he will exert the highest power of medical men, that of making absolute diagnosis, in which are always con-

tained the prognoses of diseases. The conditions of the problem of recovery or death are, that he shall do his duty, or leave it undone; in the former, the issue is in recovery; in the latter, death. When will our art reach to the height of excellency?

484. I hope, my dear young Gentlemen, you will rely, then, upon veuesection as the most effectual, and indeed, only trustworthy power vouchsafed to you in these trying circumstances; and I pray you in executing this delicate task, to observe the following methods: Let the woman be raised in bed, upon pillows that may support the trunk in a highly inclined position, though not in an absolutely upright one, unless she be still possessed of considerable strength. Select the most proper, which is the largest and most turgid vein in the arm, and open it by a free incision, to produce a copious jet. A clear, bright light should invariably fall on the patient's countenance, to enable you to judge by its hues and its psychological expression, of the influence of your operation, during the time while it is in progress. You should invariably require such a light, since the progress of your operation may be prolonged, or suspended, or arrested, or renewed, by inferences drawn from the tints of the face, and the state of the features. The design of the operation, in childbed fevers, should never be to merely fulfil some purpose of presumed expediency, to lessen a little some pain, to diminish a somewhat troublesome cephalalgia, or a vexing heat. It is to be done with a view *jugulare febrim*, by impressing upon the motive powers of the circulation such an overmastering influence, that the inflamed tissues may afterwards successfully resist the overbearing power of the general vascular reaction, which was first aroused by them alone. This impression you will not and cannot make, by timidly shrinking from your purpose, at the first observed signs of the coming influence you sought to obtain, but which is only coming and not yet come. Stop the jet now for a moment, to retard the arrival; and, drawing away one or more pillows, allow the head and trunk to take a lower inclination; then let the jet fly again, and with your eyes on her countenance; by inquiries addressed to her, and by your hand on the pulse, judge whether the influence is now come in truth and in power. If you will be but bold, resolute, resolved, as Gordon charges you to be, you will overlook the whole scene, and perceive that the heads of the monster are cut

off at one blow; or that, as Legouais expresses it, one head retains its vitality, and will revive more terrible than before. Leave off with 8, 12, or 16 ounces abstracted, and you may go away from the bedside saying "she will surely die;" but if you will courageously and resolutely persevere until twenty-four or more ounces, not too many more, are taken away, you may retire after your ministration, feeling assured that the duty is well done, and believing that the life of the patient will be saved. If she should not be rescued, it would not be because you had not fulfilled your professional duty as to this, the most important part thereof.

485. At the age of sixty-two years, and after a life in which my medical studies, commencing in 1809, and a copious clinical experience, and no little reading of authors, have continued until the present day, I am become immovably fixed in the conviction that for the cure of a childbed fever, bloodletting is the chief, not to say sole reliable remedy; I go further, and say that the dreadful mortality that marks the epidemic forms is discreditable to our class; and I say this founding it upon the irrecusable truth, that the disease is always a phlegmasia, and that it has a stage during which the application of the proper remedy is possible, and that only a lack of proper vigilance, or a lack of perception and judgment, too often, allows that evanescent stage to be over and passed, while we, alas! doubt and hesitate, and at length judge wrongly of the duty. Let me cite for you a strong passage from Legouais, who, in urging you to a proper, bold use of venesection says: "Nous croyons donc qu'on doit avoir pour but, dans le traitement de la péritonite puerpérale par la saignée, d'aneantir pour, ainsi dire, tout à coup, la maladie par l'emploi puissant et énergique de ce moyen. C'est l'hydre de la fable; on ne peut la vaincre qu'en abattant d'un seul coup toutes ses têtes; si l'on en épargne une seule, elle suffit pour entretenir un principe de vie qui bientôt fera renaître le monstre plus terrible que jamais." — *Op. cit.*, 23.

486. Dr. Joseph Clarke, of Dublin, *Obs. on Puer. Fever, &c.*, Syd. edit. p. 351, says: "It may be difficult in the beginning to distinguish puerperal fever from accumulations of feces in the alimentary canal, especially if joined to an ephemera or weed." If

the symptoms continue beyond twenty-four hours, and after the operation of purgatives, then "I consider the existence of puerperal fever absolutely ascertained, and it will be found to prove fatal to a great majority of those whom it attacks." No wonder, because, at 361 he continues: "Venesection, which is strenuously advised by Leake, Denman, and others, I have never seen of any use, excepting in a few cases where there seemed to be a combination of peripneumonic symptoms along with the peritonitis; and even in such cases it only had the effect of alleviating the severity of the symptoms."

Dr. Walsh takes up a large part of his book in a vain attempt to show that bloodletting is not the best, or even a generally proper remedy for the disease. He argues to the abandonment of venesection, from its injurious effects in scrofulas and rheumatism, and from the abuses of venesection in various maladies that he particularizes, but which I shall not here quote.—See *Walsh on Puerp. Fev.*, pp. 28–41.

Dr. Charles White, of Manchester—*A Treatise on the Management of Pregnant and Lying-in Women*, &c., London, 1791, 8vo., has at page 217, the following remarks: "In regard to phlebotomy, especially at the beginning of this disorder, authors are much divided; some of them obstinately insisting upon its efficacy, and others as warmly rejecting it. That some women may be subject to such inflammatory disorders during their lyings-in as may require bleeding, cannot be denied; but cases of this kind are not very common in the present age, especially among those who inhabit large towns. In the puerperal fever, however, which generally, sooner or later, affords striking symptoms of putrescency, we should be extremely cautious how we do anything to debilitate the *vis vite*, to weaken the circulating powers by unnecessary evacuations, or waste the strength, which may be wanted to support the patient under looseness and vomitings, &c. &c.;" and this is the way he discoursed on a topic he did not understand. Yet he is a man much quoted for his wisdom and experience as a practitioner of physic. And can it be that men in our profession, men having the very same sort of eyes, do see the same object green, black, or white, according to the point of view from whence they look upon it? yet, surely no one will contend that our child-bed fever is like the chameleon in Merrick's rhyming story.

How various are the views and opinions of men, upon the very same subjects. The illustrious Boerhaave thought it a most serious thing to let blood for a lying-in woman, and not to be attempted except under urgent necessity. The genius and learning of Kiwisch could not save even him from the German ethnological inaptitude to adopt venesection as a remedy, except with many precautions, misgivings, and doubtings, and even then not well. Von Busch, his countryman, is also one who dreads the lancet, even in these unquestionably furious phlegmasias; and good, modest old Dr. Hulme, p. 96, says of bleeding for puerperal fever that, "the first quantity taken away should barely exceed eight ounces, but that the draught may be repeated, if required, in a few hours afterwards; but it should be always remembered that bleeding in the puerperal fever (I speak it with submission), is only to be considered as a secondary relief, though the first in point of time; and that the greatest stress is always to be laid upon evacuations by stool. In the quantity of bleedings, therefore, allowance should always be made for these evacuations, which must, at all events, take place, and if too much blood be taken away, it may weaken the patient so much as to prevent her supporting the other evacuations." To me, it seems that such reasonings as these are fit to excite only a smile. I seem to discern in them the dim and ghostly shadows of the defunct milk-metastasis dogma, wandering abroad half seen, half hid in a humoralism, not less pernicious in its practical influences than the notions of *errements de lait*, coctions, and lentors. They implicate, as a condition of their own existence, the idea of an existing morbid entity, and they demand the absolute elimination of a substantial thing, a *materies morbi*, as essential to the cure; whereas, it strikes me that our first and greatest duty is to prevent the woman from dying outright, and so, leave these evacuations to take care of themselves. All such morbid entities should be treated as tails of the lost sheep in the nursery rhyme—

"Little Bo-peep
Has lost his sheep,
And knows not where to find 'em;
Let 'em alone,
And they'll come home,
Bringing their tails behind 'em."

I pray you pardon my humble illustration, which smells of the nursery.

487. The state of the patient is sometimes such as to balk one's intentions, and compel him, however reluctant, to desist, after taking away only ten or twelve, or even only sixteen ounces of blood. I may be so compelled by the fainting of the woman; by some perceptible alteration of the pulse, the respiration, or the physiognomy. I shall always endeavor, however, to carry out the design, by waiting for renewed power; by exhibiting some cordial; by lowering the pillows; by causing the woman to make repeated powerful aspirations of air into the lungs. But, in all cases, without exception of any, where I am wholly baffled, and compelled to stop short of twenty-four ounces, I shall retire from the chamber a disappointed and downcast man; because I fix my faith on Gordon's words; and, far more, because my own long bitter experiences in a physician's life, have proved to me that his words are words of truth and soberness. I am not to be understood as saying the patient will die, if not bled to twenty-four ounces; but only that it is to be expected she will not be cured in the end; when I leave such a person, I leave her little hoping that she shall be saved.

488. I readily conclude that some, and, perhaps, many of my renders will here call in question the reasonableness and moderation of these opinions of mine, so confidently asserted, as to the power of a venesection, to a certain amount, to check the further progress of the seinternal inflammations, and impress upon them a tendency to vanish by resolution. But all such cavillers ought to understand that, however decided my convictions of the truth of these propositions are in general, I do not pretend to answer for each particular case, since there may exist propensities in certain constitutions that should ever baffle the best directed means and efforts of art. I have spoken of twenty-four ounces as the quantity to be commonly relied upon, from Gordon's experience, and that of other physicians, and my own. But, in sec. 339, I have related cases in which I took a much larger quantity, and I now assure you that while, in a well-found adult, I should always desire to abstract at least the twenty-four ounces, I would not arrest the flow in such an individual as long as the pulse should

remain firm, and the face, lighted as above advised, should exhibit no proofs that I had yet sufficiently impressed the nervous centres, and, through them, the injection-power of the heart. I shall never bleed any patient of mine with any other purpose or from any other motive than this, viz: to enable the heart to act gently, and so, allow the organs or tissues that are tyrannically oppressed, and threatened with ruin from its overbearing force, to recover their conformable power; I mean the power of receiving and transmitting just so much blood as the physiological rate imposes it on them to do.

489. Some, nay many women that I have bled for puerperal fever, have fainted after the loss of eight, ten, or twelve ounces; which leads me to say that every instance of fainting under venesection, is not to be taken as a consequence of loss of blood. A patient of mine, whose basilic vein I was touching preparatory to the operation of venesection, fainted so badly that I entertained serious doubts whether the woman would not die in the syncope, for in the course of my life I have rarely seen a more threatening one. Many persons do faint from emotion—*à pathemate mentis* as the books express it; and you ought not too hastily, upon seeing the patient faint, to conclude that your work is done, and that it would be imprudent, and even very dangerous to proceed any further. But, even when early syncope is due to the loss, I believe, it may be stated as a fact that, after the sick person has revived, it is possible to take even a larger quantity without reproducing the syncopal state; for, after a too small bleeding, the vascular reaction, in inflammatory diseases, becomes greater than before; so that it is better not to bleed at all, than to do the work imperfectly. It is on this account that, when I stop short of twenty-four ounces, I always retire burdened with anxious doubts; but when I can conveniently abstract twenty-four ounces or more, I know that quantity must permanently affect the pulse, and diminish its frequency; whereas, a small ineffectual bleeding often causes the rage and violence of the circulation only to augment.

490. If the woman be raised as I have advised, and as is universally done, I believe, and the face be properly illuminated, observation of the physiognomy reveals the very earliest signs of the coming change of rate; so that by withdrawing one of the

pillows, and then another, to let the head and trunk become less inclined, one may, without interruption, succeed in abstracting a much larger quantity than could be spared if the person were in an erect or highly inclined attitude.

491. If, in any case, you should abstract as much as twenty-four or thirty ounces of blood at one bleeding, the impression made by such an artificial hemorrhage will, like that of considerable spontaneous or traumatic hemorrhages, be permanent. The woman is permanently weakened, and her power of expanding, and even maintaining at a present state the areas of phlogosis, is weakened at the same time—often absolutely nullified. It cannot be but that the heart shall now beat less frequently, and expel a smaller quantity into the aorta at each systole. You have seen, by a computation in a former section, that nine pounds are driven into the capillaries per minute, in certain excited conditions of the circulation. If you reduce this injection only ten per cent., or even only five per cent. by your venesection, the inflamed textures may, under its lessened violence, take occasion to recover by passing onwards now, what they could by no means previously transmit, and so could not avoid expanding the phlogosed areas by loading their conterminous capillary vessels.

492. Is it hazardous to take blood in this way that I have pointed out? It is by no means hazardous to do so, provided knowledge and attention preside at the operation. As the blood leaps from the open vein in bleeding, an instant pressure of the finger, or dossil, can instantly stop the jet, and let it fly again, to be again stopped at pleasure. With a finger on the pulse, and the eyes directed to the patient's countenance; by a question, or even by a surmise, the operator arrests or resumes the course of his operation—saying, it is enough—it is not enough—the work is done. *Jugulavi febrim.*

493. Read the following pleasing relation, in which Gordon gives a history of the conduct of two of his pupils, Messrs. Gordon and McRae, who seem to me to have become very thoroughly imbued with their master's principles, and familiar with his modes of practice: "An express came for me one night, to go to the Printfield to Mrs. Forbes, No. 62, who had been seized with the puerperal fever, which made rapid progress, and was attended

with symptoms which alarmed the patient's friends, and made them send for me. I dispatched Mr. John Gordon and Mr. Joseph McRae, with instructions how to act; and they managed the case with great propriety; for, when they had taken away about twelve ounces of blood, the patient fainted; but the young gentlemen were not alarmed at that, but waited till she recovered, when they took away other twelve ounces; and, after the bleeding, they gave a brisk purgative, which operated well, producing ten or twelve plentiful motions." Gordon, on visiting Mrs. Forbes the next day, found that both the fever and the pains in the abdomen were quite gone, and the woman well enough.

494. But, though the decision to commence the cure by blood-letting is easily come at in early stages, and in persons where the powers of the constitution have not yet fallen beneath the burden of irritating morbid impression, the question appears, for some, to be surrounded with difficulty where the attack has already grown old, or where the nervous centres are already reeling, trembling, and almost failing under their load. What shall enable you to decide?

495. Examine the pulses carefully in different parts of the body. Study attentively the heart by auscultation of its impetus. You may, often, better know the real rate of the function of the circulation by interrogating the heart, than by the most careful study of any of the other pulses. Let the patient project the arm horizontally, and hold it as still as possible, while you estimate by observing the act, the equal or unequal tension-power of its flexor and extensor muscles. I have made use of this latter test many times, and have resolved to perform the venesection, because I perceived, in the steadiness of the extended arm and hand, such evidence of muscular force as to neutralize, or even wholly dissipate objections to the operation, derived from other sources of observation; or I have desisted from my purpose, upon so observing the exhaustion of the muscular forces. I may safely venture here to say, that in those difficult questions as to the ability of a sick person to bear depletive measures, where strong inducements yet lead to desire them, one may freely decide in their favor upon such a test. The individual who can hold forth the arm and hand horizontal and steady, can certainly bear the loss of blood

by venesection when it is judged needful. Such a power is incompatible with a perishing frame.

496. Gordon, you have seen, confides in the power of the remedy, up to the twelfth hour of the attack; later than that he is willing to adopt it, but not to make confident predictions of its success. Perhaps this view is entirely sound, in so far as it relates to the cases of endo-metritis and metro-phlebitis, but it does not concur with my own experience or reasoning as to peritonitis, pure and simple. In phlebitis, as in pseudo-membranous laryngitis or croup, there is a stage, antecedent to the establishment of the exudative process, in which venesection *ad deliquium animi* operates with almost unfailing certainty to cure. But the stage of curability by venesection, in croup, is very soon past, and a character assumed with which venesection is wholly incompatible. The same may be stated of phlebitis of deep-seated vessels, and these are the cases to which Gordon's rule strictly applies. Not so in the merely serous inflammations, which, when not complicated with other organic or phlegmonous phlegmasias, pursue a gentler though not less persistent course to the term by effusion. Here, twenty-four hours, or even forty-eight hours may have elapsed, and yet leave you, after your skilful, perfect diagnosis, with a reasonable hope to arrest and repel the progress of the inflammation by a decided impression on the nerve-centres through bloodletting. I will not presume to say how long, in any case, the curable stage by venesection may extend; but I implore you ever to consider the weakness of our drugs and medicines, when brought alone, to oppose these persistently progressive diseases, and, using your own sound discretion, choose that which, under such fearful circumstances, may seem both best and safest.

497. I treated a case, in which I was much interested, as follows, and I shall relate the circumstances that preceded the attack: Saturday, April 17, 1852. Mrs. T. N., æt. about 32, in her seventh pregnancy, at the eighth month, was delivered at a quarter-past one o'clock this morning, after an easy labor, with forceps. My motive for using the forceps was, that she was seized during the labor with spitting of blood, and I could not suffer her to apply the whole force required for delivery. I therefore aided her with the instrument. Sunday, 18th. Has had a good night; lochia rather

too free. Monday, 19th. Being detained, I did not pay the visit until near one o'clock P.M., when I learned that I had been sent for on account of a severe pain in the pelvis and abdomen, which came on about 10 A.M. She lies on the back, with feet drawn up; breathes quick and short, on account of the pain in the belly, which she cannot allow to be touched, for the soreness of it; the pain and soreness, mainly, in the region of the left broad ligament and ovary. The uterus is very sore; the pain unceasing, but aggravated from time to time by after-pains. Face flushed. Tongue moist and broad. Heat of skin very great. Pulse 144. Upon much examination, I diagnosticated peritonitis, and therefore took from her arm seventeen ounces of blood, ceasing only because the failure of the pulse compelled me to stop, to my great regret. She had, shortly before her labor, been bled twelve ounces for a headache, and not long before that, lost six ounces from the arm. After the faintness the pulse was slower. Took forty drops of laudanum, and got six one-grain pills of opium, of which to take one every hour, unless much relieved. She took only one of them, and at 10 P.M. was comfortable, the pain not having returned since the bleeding. Before I had taken the whole quantity, she said it was gone. Tuesday, 20th. Slept well; pulse at noon above 100; at 6 P.M., nearly natural. The bowels have been moved by a dose of castor-oil. I consider her cured. I have no doubt the areas of phlogosed tissue were already risen above the plane of the superior strait when I bled her, by which operation I consider her life to have been conserved. Wednesday, 21st. I found she had had a wakeful night; pain in the belly, with soreness to the touch; pulse 120, and rather full; face flushed; skin warm; some (very little) milk in the breasts; tongue a little furred. Took castor-oil, which gave three operations, and at the evening visit the pain and soreness was gone; pulse calm, and the lady cheerful. I had no further trouble with the case, for she was well from that hour.

Mrs. S., in fourth pregnancy, delivered of a girl of seven and a half months, Friday, April 9, 1852. On Monday, 12th, saw her at 9 A.M., perfectly well; but at 10½ A.M., a violent chill, with intense pain of the lower belly. Lies on the back; excessively sore and tender to touch; breath short; countenance anxious; greatly alarmed. Tongue milky; no headache; pulse 132. This was

her state when I returned at meridian. Not a strong woman. I took eighteen ounces and stopped, compelled by her fainting. In ten minutes, opened the arm and removed six ounces more. Two drachms liq. morph. In evening, comfortable. Tuesday, 13th. Out of danger. Much better on Wednesday, 14th; and on Sunday, 18th, quite well again.

498. I shall next state the circumstances of a case that proved fatal, taking it from my notes of the date, Monday, Jan. 11, 1853. Mrs. S. R., æt. 32, primipara, gave birth to a healthy male child at half-past one o'clock P. M. She was of a highly nervous sanguine temperament; had threatenings of labor for about three weeks; I have seldom observed a more distressing case of rigidity of the cervix uteri. The labor occupied thirty-six hours, during which her distress was so great, and the resistance to powerful labor-pains so obstinate, that I greatly feared a dangerous laceration of the cervix, and was at times sorely tempted to incise it in order to expedite the escape of the head from the circle of the os, but at last it gave way and disappeared above the head. I extracted the child very gently with the Davis's forceps, leaving upon the infant's cranium and cheeks not even the slightest impression of the clamps, which I take to be proof that it was gently and skilfully done. Throughout the whole course of the dreadful labor, the circulation was torpid to that degree that I gave her small portions of wine and water, and could not think to bleed her.

499. She was passionately delighted with the child, which she caressed with marks of the greatest satisfaction. In all respects she went on satisfactorily, yet with strict injunctions to call me upon the occurrence of any, the least unpleasant symptom. Fourteen hours after the close of the labor, she was attacked with childbed fever at about 3 o'clock in the morning of Tuesday; but I was not called until 10½ A. M. of that day. I was busy elsewhere, and so, lost the first seven hours of a most dangerous case, which proved to be an irreparable loss. I found that the topical had now become a frightful constitutional disorder; for the pulse was 180 already, the pain and tumefaction or ballooning of the belly horrible, and the urine and lochia suppressed. The aspect of the case struck me as one of hopeless violence and progress. I raised

her on pillows and opened a vein, but she fainted so badly at nine ounces, that though I lowered the head, I was compelled to desist in spite of my urgent desire to the contrary. An hour later, I opened the vessel and drew off nine or ten ounces more, when, desisting under compulsion of the case, I applied leeches freely to the hypogastric region, which bled abundantly. The symptoms of peritonitis now abated so much as to fill me with hopes, not confidence. It was not long before the metro-phlebitic constitutional diseases, by their unmistakable presence, showed me that my remedies had in so far failed. I need not recite the various but futile measures thenceforth adopted, though with little prospect of advantage, until she died. The cervix had been lacerated as to its inner and lower posterior portion, though the laceration was only upon its inner superficies, not being torn through. The substance of the womb was healthy in appearance, except on its inner cervical portion, which was black, and softened by inflammation. The abdominal and visceral peritoneum was healthy, but upon the fundus uteri and posterior aspect, were observed marks of inflammation, with a small deposit of albumen coagulated. An ounce of serum was found in the Douglass's *cul de sac*.

500. This woman was certainly cured of a most violent peritonitis, except as to the small remainder, above noticed. She died with putrescentia uteri proceeding from the wound by laceration of the inner stratum of the cervix. The violent hysteroid symptoms that attended the recrudescence of the disease were evidences of purulent infection of the blood, or pyæmia.

501. I know not what may be your views on the subject, but I shall never consider that the loss of this woman was due to anything save the stupidity of her nurses, who criminally allowed more than seven irreclaimable precious hours to elapse, before they summoned me to observe a pulse of 180, the most miserable abdominal pain, and a tympany of the utmost magnitude, and tense as a drum-head. An early visit, at 5 o'clock, or 4 A. M., would have enabled me to perform a Gordonian venesection, by which to cure not only the peritonitis, but the traumatic metritis itself.

I pray you accept the account of another case, that of Mrs.

S. A., who was delivered of her sixth child on the same day, Jan. 11, 1853, with the foregoing. She is a very short delicate woman, with small limbs, and of a nervous bilious temperament. She was attacked on the same day with Mrs. S. R., and sent for me immediately, the attack having come on at 9 A. M. of the 12th Jan. I found her in the chill, with violent peritonitis and uterine pain, and flying pulses. The rapid abstraction of twenty-two ounces from this feeble, exhausted little woman, destroyed the disorder at a blow. It cured her, and I never had a moment of anxiety for her after I had drawn the blood away. An early venesection failed to cure the patient whose case I have related in sec. 224.

502. At 11½ P. M., January 23 (Sunday), 1853, I delivered Mrs. B. of a male child that weighed ten and a half pounds. She was well until Wednesday, 26th, at 3 A. M., when she was attacked with coldness, violent pain in the pelvis and belly, and great fever. She now spoke to her nurse, saying: "I am attacked with puerperal fever; get up and call my husband." "He can do you no good," was the hag's reply, and so, I was not informed until 10½ A. M. The childbed-fever phlegmasia was already seven and a half hours old; pulse 136; pain in belly intense; decubitus dorsal; so sore as to bear no touch on it; tympanitie. I drew away by measure twenty-seven ounces of blood from the arm, and then gave her a posset as follows: Take a tablespoonful of sherry, a wineglassful of milk, and a dessert-spoonful of sugar; pour over the mixture three quarters of a pint of boiling water, and strain the liquor—to be taken rapidly, and as hot as it can be borne (whosoever swallows this posset will infallibly sweat profusely within a few minutes after swallowing it). Mrs. B. sweated like a squeezed sponge.

At 2 P. M. she took ol. ricin. com. ʒj. syrup. rhei simp. ʒij. M. sig. The dose, a tablespoonful every hour, until the operation. At 5½ P. M. had taken only one dose. I reiterated the order, and directed mush-poultice for the belly. At 10 P. M. pulse 130, but soreness and heat all gone. Bears pressure of the abdomen and moves the legs without pain. Enema of salt and water; had copious alvine discharges, and the pulse fell to 120, whereupon she took two drachms liquor morph. sulph. She recovered, giving me no further anxiety or trouble. Will you

please to observe that these women recovered though they took no calomel?

503. On Tuesday, Feb. 8, 1853, I visited, in consultation, Mrs. * *. She had been healthfully confined on Friday night, Feb. 4. On Sunday morning, Feb. 6, rigor, abdominal pain, and ballooning of the belly. Signs of metro-peritonitis with phlebitis. I found at noon on Tuesday, the 8th, a pulse of 135. She was very weak, and complained of pain on pressure. When first attacked she had been treated with calomel and opium, and leeches to the hypogastrium. Upon consultation, it was determined that she should now be let blood at the arm. Upon drawing away fourteen ounces she had a great deliquium, which lasted a long time, but she began to recover from that moment. I had occasion to visit her only the 8th, 9th, 10th, 11th, and 12th of the month. She recovered well.

Am I warranted to infer that the venesection was the remedial measure on which the case turned? If yea, then it is not, as some have said, wrong to bleed in an advanced stage, for this lady's childbed fever inflammation was fifty-two hours old when she was let blood.

504. At p. 664, of my work on Obstetrics, 2d edit., I mentioned the case of a lady whom I attended here during a season of epidemic childbed fever, the circumstances of which made a great impression upon me, and it was so interesting at the time, that I shall beg to state some of the circumstances of it. She had reached the full term, and sent for me in the night, at about 11 P. M., when I was too much indisposed to go out. She was flooding before delivery. At 11 on the following forenoon I was again summoned, the attending physician having gone away. The os was about half dilated; the hemorrhage was still going on with alarming rapidity; but she had lost at least 70 ounces. I immediately ruptured the membranes, and when the uterus had contracted, the bleeding stopped, and the child was soon born, leaving the lady quite exhausted. Puerperal fever being prevalent in the city, I accordingly gave very earnest charges as to conduct, and particularly, as to giving early information should any signs of indisposition be presented. Two days afterwards she was attacked, towards morning, with furious peritonitis, and I was *not* called, but visited

her on my ordinary round near noon. The pain was intense; the tympanitis, the decubitus, the pulse, skin, face—all showed that she was most dangerously ill. I knew, or think I knew, that, if left without venesection, no other remedy could save her life, and I deemed it most probable, seeing how old the attack was, and how exhausted she was by the preceding hemorrhage, she would die, notwithstanding any treatment that could be devised. If doomed to die with this disorder, any person who should advise venesection, after so great a hemorrhage, could not escape blame, and even severe accusation of malpractice. I found the situation a very trying one; tempted on the one hand to yield to what would be a popular clamor, and urged on the other to do what conscience and judgment dictated to be done.

505. I bled this lady until she became faint, and had no trouble with the symptoms afterwards, as she recovered perfectly, so that in a few weeks she exhibited no signs of hydræmia, nor of any injurious effects of her illness. I think I was never more sorely beset to determine how I ought to act.

506. Baudelocque the younger, *op. cit.* 319, tells us of a very strong sanguine woman who had been happily delivered, but who, twelve hours afterwards, was seized with an intense peritonitis; violent chill of two hours long; atrocious pains of the whole belly, which in four hours became as large as it was before delivery; the weight of her chemise insupportable; pulse 110; hard, contracted; flushed face, with altered expression. She was brought into the infirmary of Sainte Marthe during Chaussier's visit. Baudelocque, under orders, took by a free orifice a pound and a half of blood. Towards the close of the operation she became pale and felt faint; but the pulse was fuller and less frequent; the pains were lessened. The tension of the belly began to diminish, and in the evening there was no further cause of anxiety. The rest of the treatment consisted in emollient poultices and injections, and a rigorous diet. We should not, according to Dr. Lee (*Lect.*, &c. 505), be deterred from bleeding because the pulse is small and contracted, provided it does not exceed 120-30 in the minute; for, in some cases, it has become fuller and stronger for the venesection. Even when it is rapid and feeble, if the local pain be very great, this is, here, a far safer guide

than the pulse; we must not be deterred from bleeding to the requisite extent, &c.

507. Dr. Hey's account of puerperal fever contains, in his 17th case, a history very similar to the above. He deferred his venesection, but finding, after the waste of many precious hours, that she would die else, he bled her freely to a bad deliquium, and cured her. Hey gives it as his opinion that, in no cases of puerperal fever is it more important to let blood than in those which occur after excessive uterine hemorrhages. And he adds, "If, then, venesection be found in such cases, not only a safe but an effectual remedy, its necessity will be less disputed in other and more favorable circumstances," p. 157. I beg you to bear in mind this sensible remark of the English practitioner; it is an excellent argument against those who are opposed to the abstraction of blood in all cases, and may confirm you in the right when, in some doubtful case, your own judgment may be vacillating.

508. How long, then, after the onset of a childbed fever is it that a physician must positively decline to attempt the cure by blood-letting? (see sec. 479.) The answer to such a question must be deferred to the special occasion, and then it will turn upon the actual diagnosis. The tendency of the inflammation is to effusion, which is its commonest term. As long as that effusion has not commenced, it appears to me both lawful and highly expedient to attempt a cure by resolution, since other terminations are mortal, and since the most powerful means in our possession to that end, consists in bringing about those modifications in the blood's force, that we commonly try to effect by diminishing its sum; that is, by venesection. I cannot, therefore, agree that any man should, *à priori*, determine the time-limits within which the case admits of a prudent and hopeful treatment by the lancet. As I have already said, we ought to hope for and attempt a resolution, as long as the exudative or secreting stages are deferred, and we must judge of the fact upon the evidence brought up by our interrogations of the patient herself, as well as of her organic functions and powers.

509. Dr. Lee (*Lectures*, &c., p. 501) says expressly: "The great point is to institute this remedy before effusion of sero-purulent fluid has taken place, or at least before it has gone to much ex-

tent; but as nothing is known regarding this, further than that it will take place at different periods in different cases, it is manifest that no general rule can be established, and that the practitioner must draw his conclusions as to the propriety of using it, from the history of the case and the existing symptoms."

It may seem very strange to you that, after all I have in this Letter written on the indispensable necessity of bloodletting in the treatment of our cases, I should confess that I am now (September 25, 1854) treating a most distressing example of childbed fever inflammation without the aid of either lancet or leech. The patient is a lady about twenty-two years of age, a very small and delicate person, of the highest sensibility, and occupying an elevated position in society. She was in her first pregnancy, through which she passed to full term on Wednesday last, September 20, when her labor began with rupture of the ovum, which was immediately followed by regular labor pains. About six weeks ago, being then residing at her country-seat, some miles from town, she was seized with what seemed a remitting bilious fever, during the progress of which I suffered considerable anxiety, for she was so ill with fever that I much apprehended fatal consequences. She was treated on the occasion, after venesection and leeching, with very mild remedies, that had a happy conclusion; so that, upon recovery, she came to her town house to be confined, under my care. In the country fever, I observed the pulse to become excessively hurried, and at the same time soft; and, as there was considerable delirium, I imagined that the blood must have suffered serious injury from the state of its *membrana vasorum communis*. Upon rising from the sick-bed, there came on rapidly an enormous *cedema gravidarum*, extending up to the perineum and labia, which were much distended by the infiltrating serosity of the blood. The urine became heavily charged with albumen, as tested by heat, and she was pale, weak, and nervous; in which state she began the labor, as before said, by rupture of the amniotic sac. Touching showed a fifth vertex position, and a hard, unyielding cervix and os. The pulse at first was not much disturbed.

The waters broke early in the morning of Wednesday. She suffered very sharp anguish with the grinding pains, and much sense of pressure within the pelvis. I drew off the urine, as she passed none spontaneously. As the head descended, I occasionally

endeavored to bring the vertex from the left sacro-iliac junction towards the left acetabulum, but without the least success. About midnight, the dear little lady was dreadfully fatigued with her efforts and sufferings, and though I had indulged the hope to see the child descend towards morning, I found the pain lessening in force and frequency, until it gradually became null. Nevertheless, constitutional irritation, connected, as I supposed, with the inward tension, packing, and jam of the head, which seemed to be locked by a sort of *gomphosis* in the pelvis, excited my alarm. The sacrum was flat, and it was impossible to cause rotation either forwards or backwards, the cranium being fixed in a manner to cause the sagittal sutures to cross from right to left. The pulse became greatly hurried—alarmingly so—and, after vainly attempting, by means of Davis's oblique forceps, to cause it to rotate, and after many efforts made with the hands, I gave it up, and was compelled to resort to the forceps. Carefully adjusting a pair of Dubois's powerful forceps, constructed for me under M. Cazeau's direction, by Charriere of Paris, I concluded that I should now certainly extract the head; but no skill nor force of mine made the smallest impression on it. I might have as well tried to move a church with the instrument. I did not move it one iota; and her distress was so great, notwithstanding that she inhaled ether, I was compelled to give it up. By this time, the pulse was running, and she was bathed in clammy sweats, and seemed to be passing into that state of nervous shock which I have seen in labors, but have not seen recovered from. The question now was to deliver her, or see her perish undelivered, a finale that could not be long deferred if undelivered.

It was necessary to use the Holmes's perforator, with which the encephalon was discharged, and then, the cranium collapsing, I expected readily to draw it away in some six or ten minutes, with Meigs's embryotomy forceps. Would you believe that, though I could take a perfectly good hold of the part with the jaws of the instrument, so as to enable me to exert great force with two very strong arms, I was busied for two hours and a half in getting away the head, after which the trunk came away also, after a hard pull.

By this time she was utterly exhausted, and more likely to die than to live six hours, the pulse being 180 to 200 per minute, and

the breathing very bad. The foetus was extracted about 5½ A.M. of Thursday, September 21. . . . She got a full anodyne of morphia, and was not moved for many hours; carefully bandaged; wine whey and arrowroot, with a little brandy; cool water to drink.

All day of Thursday the pulse was flying; the lower parts enormously swollen; immovable in dorsal decubitus; pains in hypogaster. By aid of the morphia, at night slept a little, not having closed the eyes the preceding night. On Friday morning, 22d, unmistakable signs of peritonitis; that is, rapid pulse; pain on pressure or coughing; enormous tympany; retention of urine; great weakness; womb hard, and strongly drawn by broad ligament and round ligament towards the right iliac region. There was membranous inflammation, with phlegmonous nucleus, for who can doubt that this sore and tired womb, and the tissues within the pelvis, were in a state to be called phlegmonous inflammation. As to phlebitis I could not speak, nor can I now with confidence. Her life has appeared to be suspended upon a thread during Friday, 22d, Saturday, 23d, and Sunday, 24th. To-day, Monday, 25th, I cannot detect the slightest hysteroid. The pulse has become slower and fuller. The vast tympany has almost disappeared; pressure with hand is tolerated; the legs can be drawn up and pushed down without pain; the urine flows abundantly; the oedema gravidæ is gone; no headache; tongue, that was furred from 22d to 24th, is much cleaner. Spirits good and cheerful, so that at this moment, 12 o'clock P.M., I feel a considerable confidence in the hopes that are raised of her recovery. She herself thinks she will recover.

Why, do you ask, did you not bleed her? I can by no means express the fervent desire I had to open a vein in the arm and remove twenty or thirty ounces of blood, but it was impossible to think of it under the circumstances. I gave her wine-whey, and chicken-broth, and arrowroot, and brandy and water, with repeated doses of sulph. morphia; also, a small potion made with turpentine. I believe I may now expect her to escape, but, I confess, I scarce know how or why. I have given her many baths with hot spirits and water, the nurse using sponges squeezed in the liquor to bathe the whole surface underneath the bed-covering. The case seems to me very much like one told by

La Roche when he practised at Geneva. You may read it in my section 566, where I have quoted it. I did not use his powders of bark, however. Tuesday, 26, 9 P. M. She will sink. Wednesday, 27. The lady expired at two o'clock this morning; very few persons can be expected to survive these childbed fever inflammations, whether accidental or unavoidable, when the circumstances forbid a resort to bloodletting.

510. With these remarks, in making which I seem to have unbosomed myself upon what I deem one of the most important questions in obstetric practice, I shall bring this long letter to a close, but not without expressing an earnest wish that my Students would give to the subject a very large and earnest attention.

C. D. M.

LETTER XX.

ON LEECHING, AS A REMEDY FOR CHILDBED FEVERS, WITH A COMPARISON OF ITS ADVANTAGES WITH THOSE OF VENESECTION.

511. I THINK there are a great many physicians who have conceived an aversion to bloodletting as a remedy in childbed fevers, but who feel no apprehension at all as to the use of leeches, which they suppose to be as capable as venesection of allaying the inflammatory turgescence of the affected parts. In consultations, you shall sometimes meet the greatest opposition to your proposal to bleed the in-lying woman, on the part of Doctors who fear the click of a spring lancet as a "hurt fowl, or a struck wild duck" fears the "report of a caliver;" but, if you say "leeches," those very people meet you more than half way; and though they are thunderstruck if you propose to take some ounces from the arm, they entertain not the least feeling of concern about the noiseless and unseen absorptions of some dozens of leeches.

512. No person doubts that the leech is, in some instances, a very desirable adjuvant, nor that the blood it draws does lessen the amount of the balance left in the circulatory system; neither does anybody deny that a great leeching does let down the constitutional excitement, in fevers. But this is not the important question here, and I anxiously solicit you to consider that while both methods, venesection and leeching, may produce such results, the methods are not alike in either celerity or power; since you can, in the course of five minutes, bring about by a venesection, and with greater certainty, an effect which it would require some hours to accomplish with leeches, with great inconvenience, if not pain and serious risk. If this is true, and if, when you take eight ounces of blood by the lancet eight ounces are lost to the economy, and if the same is true where you take eight ounces by leeches, then I see not wherein the leeches are more desirable.

513. A venesection is both a much neater operation, and devoid of many objections, that attend the bleeding by leeches. There is no fatigue; it may be arrested, suspended, and renewed again, in a moment, without inconvenience. You can stop the jet for a minute, or for ten minutes, and let it fly again, after a few minutes' examination, during which you are weighing the question of how much, what appearance, or what is the effect, so far. In leeching, the abdomen must be uncovered; the punctures are exceedingly irritating, and, for some people, very painful; the long-continued rest, in an unchanged position, exhausts the woman's patience; there is water spilled about her person and dress, and she is moreover smeared with blood, and afterwards incommoded by the necessary dressings. In twelve hours after a leeching, the patient may complain, on being touched, of pain that you know not whether to attribute to the internal disorder, or to the erythematous areas that always follow the bites of leeches, and so you are liable to be a little in doubt about your diagnosis. In fine, you cannot venture to take so much, and you cannot get it so speedily, as you would draw away from an opened vein; and if you could do it, it would be of less influence upon the blood's force—in motion. My own clinical experience has convinced me that leeches ought not to be used where we durst employ venesection, and so, speaking generally, I greatly disapprove of them in the treatment of our cases.

514. There is a false impression made upon the mind on this matter: leeches are usually employed with a view to secure the effects of a direct depletion of the affected tissues. Could there be a greater mistake than to suppose that leeches on the lower belly should exhaust the capillary vasa vasorum of a uterine phlebitis or a metritis, or even of a pure and simple peritonitis? You are to observe that the peritoneal inflammation you dread, is that of the mesorectum, mesocolon, epiploon, and intestines generally, as well as the whole intro-pelvic serous tissue. What possible direct relation, then, can there be betwixt the capillaries of the corpus mucosum cutis of the lower abdomen, and a congeries of capillaries supplied from the inferior mesenteric, or the uterine, or the ovaric arteries? There is no such anatomical relation; and I believe that in leeching for childbed fevers, you could apply the leeches as usefully to the arm, or the calf of the leg, as to the

hypogastrium of the woman, where they are usually set by operators.

515. My mind has long been impressed with a sentiment that amounts to the force of a conviction, that leeches, when used during a strong febrile excitement, for the subduction of a topical engorgement, do to a certain extent, rather promote than lessen the hyperæmia of the part; at least they do so, provided the abstraction is not carried so far as to cause a great diminution of the injection-force of the heart with coincident lessening of the quantity transferred, per minute, by the force of the systoles. I am equally persuaded that, provided, in fevers, leeches are indicated as topical depletives, they should follow, and not precede a bloodletting at the arm. When a full venesection appears to be insufficient to disengage the phlogosed areas, then a topical bleeding seems more likely to be useful. But upon the whole, I have a positive distrust of and dislike for them in all inflammations accompanied with high fever.

516. The same opinions I have above expressed, you will find have been sustained by the younger Baudelocque, at 332; and though Gase, in his *Dissertation on Puerperal Fever*, p. 285, a man much afraid of the lancet, praises the leech, he applies it to the vulva or the perineum—operations, either of them, in the highest degree objectionable in such cases as these. The woman is too sick to be worried in this disgusting way.

517. D. Gooch, p. 47, observes that, in the treatment of acute inflammation in the vital organs, it is common to consider local bleeding as a milder means than general bleeding, and to postpone it until the stage for venesection is over. "To me it appears that they are calculated to effect two different objects, both of which are necessary at the beginning of this treatment; the one to reduce the violence of the general circulation, the other to empty the distended capillaries of the part. As long as the pulse is quick, full, and hard, it is in vain to take blood from the affected part; if we could completely empty its engorged capillary vessels, they would be instantly gorged again whilst the heart and arteries are injecting them with so much violence." Now these observations of Gooch are admirable, except for the false impression they are likely to produce upon the

unreflecting reader, as to the ability of the physician to take blood directly from the inflamed parts, by leeches; a thing he cannot do, because there is no blood-connection between the capillaries of the hypogastrium and those of the womb, or those of the meseraic circulation.

518. There are many very able physicians whose opinions are wholly at variance with mine, above expressed, as to the uses of leeching in our cases, and among them there is none of higher authority than Dr. Meissner. In *Die Frauenzimmerkrankheiten*, vol. iii. 820, he contends, it is true, as he formerly showed, in Siebold's *Journal für Geburtshulfe*, Bd. vi. St. 2, S. 306, that four ounces of blood, taken by leeches, in a childbed fever, from the affected parts, are more efficacious to the cure than twelve ounces drawn from a vein. Notwithstanding the great respect I have for Dr. Meissner's learning and wisdom, I cannot agree with him here, nor believe his assertions to be based upon clinical experience, but only on a ratiocination, and should willingly agree with him in the opinion, were it possible, which it is not, to take the blood by leeches from the affected part. One is deceived who thinks he draws it from the affected parts because he is taking it from the skin over them or above them.

519. M. Dance, *Archives Gén.*, 199, t. 19, is not unfavorably disposed as to the use of leeches, which he also seems to regard as capable of producing direct depletion of the engorged parts. Of this you are to judge. Yet, he says the proper treatment of uterine phlebitis is to be effected by the early and energetic use of antiphlogistics, and that venesection depletes more effectually than any other method; but we should apply leeches afterwards, and adjust them first *au niveau*—that is, opposite to the parts inflamed, and rise, afterwards, in applying them, higher and higher as the inflammatory areas extend upward—their usual direction.

520. Another writer of great authority and merit, distrusts bloodletting, particularly in epidemic hospital cases—I mean, Dr. Robert Collins, of Dublin. In his *Midwifery*, 393, he says: "Where the state of the patient was such as to encourage a general bleeding, we used the lancet; I am satisfied, however, that in *hospital*, the immediate application of *three or four* dozen of leeches, followed by the warm bath, in which the patient should remain

as long as her strength will bear it, will be found, in the great majority, the most judicious means of removing blood."

521. Such are Dr. Collins's remarks; whereupon, I shall only ask you to consider how much blood would be removed by three or four dozen leeches, the patient being afterwards placed in a warm bath; and whether, if a certain quantity is to be removed, it would not be as promptly, and as safely and pleasantly done by the lancet?

522. Think of the meaning of the word centro-peripheric, and ask yourself whether all your means of subduction for the organic lesion are limited to the peripheric, while you have no interest in the central influences. If such is your conclusion, I must wholly differ from you. I strove, in my last long Letter, in explaining the uses of bloodletting, to persuade you that, wherever you can control the heart's injecting power, so as to modify it according to your wishes and judgment—in all these cases you will be likely to cure your patient. Both Dr. Collins and Dr. Meissner, as well as Gasc, and the leechers generally, appear, in controlling the disorder, to expect to effect a cure by impressing the periphery only—overlooking, as I consider them to do, the centre, which it is vastly more important, as well as more easy to modify or bias according to our will.

523. Legouais, *op. cit.* p. 38, says, that topical bleedings, particularly by leeches set to the vulva and on the abdomen, in numbers sufficient to abstract a considerable quantity of blood, have, under his own observation, been too many times followed by success to allow him to doubt of their efficacy, especially in hospital, where reasons existed to make one fear a general bleeding. But, outside of these cases, general bleedings appear to have always been indicated in a more precise manner, because their influence on the economy is far more marked in a case where the inflammation is so extended, and so, convenient to abstract promptly and with certainty the entire quantity deemed necessary; a quantity much more easily to be appreciated than one withdrawn by leeches, &c. &c. Many of our brethren are used to direct cupping-glasses to be applied to the belly, in their cure of childbed fever. This is a practice I have never yet adopted in a single instance in my whole life as practitioner. It has

always appeared to me that such treatment is a mere barbarity, seeing the relaxed condition of the abdominal integument soon after childbirth. The same motives that lead me to disapprove of leechings, apply to the case of cuppings; but with far greater force in the latter.

524. Upon the whole, then, I wish you to regard me as one very distrustful of leeches and cupping, believing them likely to mislead us, by begetting within us a false opinion that we can by them directly deplete the affected areas—which we cannot do; and also as seducing us to suppose that, because we draw blood only from the capillaries, we are not, in reality, bleeding the woman, but only leeching her. But, I repeat it, I will not presume to aver that, after a good bleeding, it may not be, sometimes, judicious to apply leeches over the ligamenta rotunda as they pass along through the abdominal canals; but I do ask you to think well and distinctly, whenever you are halting between the two opinions as to bleeding and leeching the childbed fever patient; and I shall close my communication with these requests.

C. D. M.

LETTER XXI.

EMETICS IN CHILDBED FEVER—REPORT OF THE COMMISSION ON DOULCET'S SPECIFIC—DR. JNO. CLARKE'S VIEWS OF THE DOULCET TREATMENT—DR. WHITE AND DENMAN FAVOR A MODERATE USE OF THEM—FERGUSON'S OPINIONS—DE LA ROCHE'S GUARDED RECOMMENDATION—TONNELLE'S EXPERIENCE, WITH CASES.

525. It is now a long time since the Treatment of childbed fever by emetics was considered by persons high in authority as a way almost specific to cure it. Dr. Stoll, of Vienna, seems to have abhorred the use of the lancet; and the reason of it was that he took an erroneous view of the malady, which he chiefly considered to be a bilious disorder, although capable of assuming an inflammatory character. Stoll, looking upon the disorder as bilious, was led to confide in the use of emetics; for he thought that if the inflammatory constitution of the attack did depend upon bile, the most efficacious of antiphlogistics must be an emetic, capable of casting the life forth—which seems to me a lame way of reasoning. But, leaving out of question Dr. Stoll, and all the lesser lights on this topic, let us refer to a *Report, by order of the Government, upon a Memoir containing the Method of the late Dr. Doulcet, Docteur-régent of the Faculty of Paris—one of the Physicians to the Hôtel Dieu—in the treatment of a Disease that attacks lying-in women in that Hospital, and which is known under the denomination of Puerperal Fever.* Read at the sitting of the Royal Soc. of Medicine, Sept. 6, 1782.

This report is printed in *Hist. de la Soc. Roy. de Méd.* 1780 and 1781, vol. iv. p. 254. After describing the disease, and relating the history of some *post-mortem* researches, to show that the malady in question was puerperal fever, the account proceeds as follows, at page 255:—

“Equally rebellious against the efforts of art and the resources of nature, this swift disorder has constantly resisted the most wisely applied remedies, whether selected as means to obviate inflammation, or to turn aside the humors from the viscera of the lower belly, or to recall the milk to its real path, to oppose a tendency to putridity, or to procure salutary evacuations. Everything has been tried—everything has failed. Ipecacuanha itself, which is the basis of the present method, met with no better success than other remedies, until, as chance would have it, M. Doulcet was present at the very moment when the disease made its attack in a woman recently delivered. It began by producing vomiting. M. Doulcet, immediately seizing upon the indication, ordered fifteen grains of ipecacuanha, which the patient took in two doses, that were repeated the next morning. The medicine operated upwards and downwards. The evacuations were followed by a notable diminution of all the symptoms. The looseness was kept up by an oil-potion, with two grains of kermes, and the patient was saved.”

The account next goes on to show that, enlightened by this success, M. Doulcet felt the importance of the moment, and the necessity of seizing it before the malady should have time to settle itself; and, accordingly, the attendants were directed to give the doses, day or night, at the first moment of the assault. The success was everywhere the same; so that, during four months that the epidemic raged with fury, near two hundred women were restored to existence. Only five or six persons, all of whom refused the emetic, fell victims to their own obstinacy. The *post-mortem* appearances presented by their dissection showed that they had perished with the puerperal fever. In fine, the method in use at the Hôtel Dieu, and which has not failed since it began to be employed, consists in seizing the very moment of attack to give, without loss of an instant, fifteen grains of ipecacuanha, in two doses, at an interval of an hour and a half; to repeat it the next day, whether the symptoms have changed or not; and, if they persist obstinately, to reiterate the same dosings as much as three or four times, according to their obstinacy. In the intervals, the effect of the ipecacuanha is sustained by means of a potion compounded

of oil of sweet almonds, an ounce of syrup of guimauve, and two grains of kermes mineral. The drink is plain flaxseed tea, or scorzonera tea, sweetened with syrup of guimauve. Towards the seventh or eighth day of the disease, the patient takes a mild laxative, which is repeated as circumstances may demand.

This new method, which might be called the specific method, was published in a paper through the Faculté de Médecine of Paris, and, as I have said, the news of the discovery was hailed with joy; not because no physician had ever before that time administered a dose of emetic medicine, but because of the signal success that attended Doulcet's particular method, "to whom it was reserved," to use Doublet's language, page 204, "to give to the exhibition of emetics in puerperal fever a greater value, and to find a mode of using them far surer and more beneficial than any that had been tried before his time."

526. Such, gentlemen, is an account of the report on Doulcet's specific for childbed fevers, the announcement of which made almost as much noise as Sigault's operation, or Jenner's discovery. Dr. Doulcet was looked upon as a benefactor of mankind, and every *gobe-monche* of a doctor, from Landsend to Archangel, I presume, laid to his soul the flattering unction that a cure for the most terrible of diseases was at last found. This was in 1782, just seventy-two years ago; and to-day nobody thinks of giving an emetic in a childbed fever inflammation.

The treatment or method, which was anxiously and thankfully taken up everywhere, was soon dropped, and, as commonly happens, its disappointing results brought it into utter contempt and after-neglect. Dr. John Clarke, *Observations on Puerperal Fever*, &c., Sydenham Society's edition, thinks a repetition of vomits, on the plan of Doulcet, has been attended by obvious disadvantages; and M. Doublet, *Nouvelles Recherches sur la Fièvre Puerpérale*, p. 206, says: "Emetics, in English hands, have never had the same success as in those of M. Doulcet and his colleagues at the Hôtel Dieu at Paris;" and he mentions that Dr. Fothergill, the learned and benevolent London physician, admitted he and many other English practitioners had often exhibited emetics without obtaining the same beneficial results as Doulcet, and that Dr. Fothergill's paper in the *London Medical Journal*, vol. iii., informs us that the method (Doulcet's) ought to be considered

rather as a conservative than a curative one; but that, far from detracting from Doulcet's merit, the circumstance ought to enhance it in the opinion of such as think it best to err on the right side, and that it is more desirable to prevent than to cure diseases. I perhaps spoke too strongly when I said just now that nobody thinks of giving an emetic in childbed fever nowadays; it would be more correct to say that they are rarely resorted to out of Paris, where, I understand, they are still sometimes administered in the hospitals; and Dr. Ch. White, of Manchester, *Treatise on the Management of Pregnant and Lying-in Women*, page 195, states the conditions of headache and pains in the back, with swelling, pain and tenseness in the lower belly; nausea, vomiting, diarrhœa, tenesmus, strangury, thirst, white or brown tongue; or any of these symptoms, as motives to give a gentle emetic of ipecacuanha, &c. Denman also recommended emetics as part of the curative method; but White and Denman are long ago dead.

527. Dr. Ferguson, *op. cit.*, commences at page 202 his remarks on the method by emetics, and signalizes the disappointment that followed in succeeding years the flattering hopes excited by Doulcet's success. Dr. Ferguson, whose notions on puerperal fever are inextricably interwoven with his ideas of vitiated blood, thinks that where there is a bilious complication they should prove invaluable as remedies. "Where there is an obvious struggle in nature to cast off the poison through the liver and intestinal canal, and where that effort is marked by nausea, bilious vomiting, bilious suffusion, or bilious diarrhœa, it will be requisite to resort to Doulcet's treatment." P. 204. He advises that they will be injurious where none of these symptoms are present, and says he has not had many opportunities of exhibiting emetics, chiefly because he saw the patients late in the disease, the remedy being useful only in the very early stages.

528. Tonnellé's celebrated publication strongly vindicates the propriety of using the ipecacuanha. You may find his observations upon this subject in his fourth and last article, which is at p. 184, vol. xxiii., 1830, of *Archives Gén.* He says, that persons who are preoccupied with the "exclusive idea of inflammation, receive, with distrust, everything foreign to the range of ordinary antiphlogistic measures. Trained up in the deceptive and mis-

chievous notion that inflammation is incompatible with anything but bloodletting and emollients, we reject every other species of treatment as in opposition to our preconceived ideas of the nature of inflammation, whose nature, meanwhile, is utterly unknown to us." You here see how strongly Mr. Tonnellé takes ground, before he commences to make his exhibit of the power of emetics in the cure of childbed fevers; and you will bear me witness that I am not desirous to keep in the background the sentiments of so able a man as he was, because they are opposed to my own. Indeed, I wish you to observe that he says, that although sanguine evacuations do combat inflammation in one of its principal effects, they do not attack it in its cause. Now, asks he, are there no therapeutical agents that are capable of fulfilling this object? Are there not agents, by means of which it may be annulled or neutralized by exciting, in the economy, certain diversions, like those we see brought about by the power of nature every day?

529. Such are Tonnellé's queries, which I leave you to judge of, but with this remark only, viz: if they have no greater influence upon you than they have had upon me, you will promptly answer no; there are none such of any value in clinics.

530. I have in these pages so often pointed out the tendency of these childbed fever inflammations to attain to their natural term in effusion, that I somewhat apprehended you might accuse me of believing a spontaneous resolution to be a thing impossible. While I am far, I hope, from such an extravagance, I avow my belief that one of these inflammations, fairly aroused, and reacted upon by the fever it has itself developed, is far more likely not to end in resolution, if left uncared for, than otherwise. But, a termination in effusion or gangrene, may be assumed as in general a mortal result. It is true that Tonnellé gives a series of five cases cured by ipecacuanha, administered in accordance with Doucet's plan, as nearly as possible. Yet the last two cases were treated at the same time by means of leeches. He adds a series of six other cases cured by ipecacuanha, aided by certain other measures, as leeches, &c. &c.

531. I freely admit that these ipecacuanha cases were cases of the disorder, and that they recovered without the intervention of general bloodletting. Yet I am not willing to infer from the facts

that the treatment was either so safe, or so prudent in all respects, as it would have been had the lancet been wisely employed; for, I take for granted, that since the women did recover from the disorder, they would not only not have been injured or hazarded by venesection, but that they would have been far better managed. They could have borne a venesection, else they would not have been cured by ipecacuanha. Tonnellé's eleventh case, p. 200, was that of Lori——, æt. 26; sanguine but vigorous constitution; attacked five days after accouchement with puerperal fever. On the 6th, violent pains in hypogaster, especially in left iliac fossa where slight pressure is intolerable. Took the ipecacuanha, and vomited enormously of bilious matters, with prompt relief. Evening, no fever, but pain continued in iliac fossa, whereupon she had forty leeches there that bled copiously. 7th, the belly still somewhat sore, but the uneasiness disappeared in a few days, and she went out in good condition. Such was the case, which gives no state of the pulse and other important circumstances. Upon the whole, I should have felt safer to bleed than trust to ipecacuanha in such a patient.

532. As to his Chap. IV. on emetics (in childbed fever), where Tonnellé gives five cases cured by ipecac., as he says, though two of them were treated with leeches also, and where he adds six others treated by ipecac. and other remedies, I cannot look on these as fair instances to prove the power of emetics, at least to such a degree as to lessen one's reliance on the best of the antiphlogistics and venesection; nor can I regard it as conducive to the advantage of practical science that any man should set up such a stone idol, and worship it, to the disparagement of better and safer gods.

533. And here I must quote from that most sensible physician, De la Roche, *op. cit.* 196, who says: "I believe, therefore, that emetics, and especially antimonials, may be advantageously employed in puerperal fevers. But let no man deceive himself as to their mode of action, and, attributing the success to the evacuations they procure, imagine that drastic purgatives that have an entirely opposite effect on the sanguiferous system, can be substituted for them. Care should be taken, also, in exhibiting them, not to make large doses, which excite violent efforts to vomit,

lest strong and sudden compression by the abdominal muscles, of the inflamed textures, should occasion dangerous irritation."

Let any one read Tonnellé's tables, at the end of his paper, intended to show the number of the cured by emetics, and consider also the juvantia by which they were supported or reinforced, and it seems to me he will scarcely, after all, place great reliance on Doulcet's practice. But, perhaps, the most cogent reason to distrust that method, may be found in the fact that it is now rarely confided in anywhere. I would by no means, however, dissuade you from employing the ipecac. in those instances where a bilious or mucous saburra might lead you to fear bad gastric complications of your childbed fever inflammations. You should, as an act of duty, make yourselves acquainted with Dr. Stoll's views of the bilious character of the whole disorder; but, I hope you will never suffer yourselves to be led astray from the simple, absolute truth, that there is no such thing in nosology as a childbed fever, as the title of this book would erroneously lead a person to suppose; but only a group of disorders, that, whatever their particular seats, are properly to be called childbed fever inflammations.

534. In my next Letter, I propose to discuss with you the subject of mercurials, as remedies for childbed fever; and I now rest, with much respect,

Yours, &c.,

C. D. M.

LETTER XXII.

MERCURIALS IN CHILDBED FEVERS, WHETHER USED IN THE FORM OF CALOMEL OR BY INUNCTION.

535. It seems to me that the doctors have, in a small degree, gone crazy on the subject of calomel and mercury, so that when a man is sick, calomel; broken leg, calomel; pleurisy, calomel; encephalitis, calomel; scarlatina, calomel; yellow fever, calomel; ophthalmia, calomel; dropsy, calomel; syphilis, calomel; amenorrhœa, calomel; menorrhagia, calomel; strong, calomel; weak, calomel: as to cholera, I doubt whether of the sixty millions of persons who have died with it since the Jessore epidemic broke out in 1817, $\frac{8}{10}$ ths of them did not reach the banks of Styx, carrying in their stomach the current metal that rejoices in the name of that god who marshals souls along the way to Hades. In ancient times, men went down there with a silver obolus in their mouths to pay the ferryman. Perhaps Charon is dead now, and it is found that *argentum vivum*; *lucus a non lucendo*, is the very thing for dead men. Most certainly, there be few dead men unprovided with a treasury of the subtil metal.

But mercury is one of the reliances of modern practitioners for the cure of childbed fevers; and, as you with the rest of the medical world, are likely to use it, let us consider what are the circumstances to which it is adapted. I am far from even hinting that there are no circumstances of a childbed fever in which it may not be prudently and usefully employed. I do only complain of its too much use.

536. It is very long that I have been no lover of the calomel practice, as it is used in the United States, where I feel assured that it disappoints a thousand great expectations, usurps often the place of better remedies, and in a vast number of instances

produces great distress, and sometimes irreparable mischiefs: nevertheless, mercury is a strong medicine, and is not always to be dispensed with in our practice. But what are the powers of mercury, used as a medicine? The generality of practitioners in this country would probably reply to this question, that it is an alterative, while many would regard it as anaplastic, or a medicine capable of opposing the development of that state of the blood that leads to inflammation. Some would insist that its therapeutical value is in a main degree allied to its power to promote the hepatic activity, and other some look to its direct impression upon the nervous system, as they say, for the beneficial results of its operation. It is hardly requisite to refer to its well known cathartic property, or its salivating, in other words, poisoning powers.

537. But mercury is a specific; and, perhaps, if you add cinchona, one of the only two specifics we at present possess; unless, indeed, you prefer to regard all medicines as specific, either for the economy at large, or for particular organs or special functions: in the latter sense, ipecac. is specific in its power on the stomach to provoke emesis, and rhubarb and other aperients and purgatives are specific for the bowels, to produce alvine evacuations; as mercury is a specific for the production of ptyalism. As to the operation of mercury commonly assumed to be alterative, we do not know what we precisely mean by that word; the idea, a confused one at best, being empirically derived, and not rationally. We cannot even know, according to Dr. Renouard, *Hist. de la Méd.* &c. &c., tom. ii. p. 434, precisely what we mean by the anti-syphilitic power of mercury, and we can only have an experimental or empirical knowledge of the fact or consequence. "I know not," says that ingenious author, "how a minim quantity of a mercurial salt compels all the symptoms of a syphilis to disappear as by a charm, in a multitude of cases. But what matters it to me or to the patient? What is essential to us is, to know that the effect will take place, and the circumstances in which it will take place." * * "Without seeking, then, to know why mercury dispels the syphilitic symptoms, or why vaccination prevents smallpox, or cinchona cures intermittents, &c., I shall employ these means whenever they are indicated, with as much certainty and reason, as I should use venesection in a phlegmasia."

435. Pray ponder for a while, upon Dr. Renouard's words above cited, and then, after what you already know of the nature, seats, and propensities of a childbed fever, say whether, instead of resorting, in it, to mercurials with the same confidence as in syphilitic disorders, whether mercury, used as it has been used, is not rather to be classed among the perturbative, or shock-remedies. M. Renouard, *op. cit.* 535, says: "The practitioner, after exhausting all the rational means that science has placed within his reach without any satisfactory result, due to some idiosyncrasy or other inexplicable circumstances, the practitioner, I say, has recourse to an indirect method, by which he proposes to impress upon the whole economy or only upon the diseased part, a shock (*secousse*), in order, thereby, to produce an advantageous and curative perturbation. This was the object of the old methodic sect, in the invention of their metasyncritic circle; such is the end proposed every day, when we order our patients to the sea-bath, to travel, to visit mineral springs, &c. &c."

538. I am inclined to believe that the trust in calomel, as an anti-childbed fever medicine, arises, in part, from this over-anxious desire to do something useful, even at a guess; to perturb either the whole economy, or the part, in hope to find, after the shock or disturbance, a new sort of action, or some different vital status induced, somehow, we know not how or what. The idea of the word *alterative* appears to me to be this, and nothing more; and I am greatly mistaken if this word *alterative* is not uppermost in the minds of physicians generally when they are prescribing doses of mercury and mercurial application for the treatment of childbed fevers; else I am so dull and stupid that I cannot comprehend them even when they "touch the bile," act on the liver, and do a thousand other queer things with their universal dose. Of course, I except those instances, where calomel, &c., are ordered with a sole view to their aperient or cathartic qualities, for I can understand a purgation as a something real, and even very salutary. I have, in sections 467-8, related some cases of my own, wherein I specially called your attention to the omission of mercurials from my method; and I showed you, in section 239, that I used calomel freely enough. I was, formerly, cruelly and flagitiously devoted to calomel, but I have learned better now.

539. Dr. Robert Collins, *op. cit.* 395, generally combined ipecacuanha with calomel, because not only of the useful therapeutical influence of the ipecacuanha, but also because "the ipecacuanha, where it does not sicken, seems to have the best effect, *preventing* the calomel exciting irritation in the bowels and producing moisture over the surface." Now, this is a sample, as I take it, of the exhibition of mercury with a view to its alterative or shock-power; for the calomel appears to be attended with the great inconvenience, if not risk, of irritating the bowels, therefore it must be guarded with ipecacuanha. Would it not be safer to leave it out altogether, and take the ipecacuanha alone, as the ipecacuanha serves as a guardian angel to obviate the mischievous attributes of the metallic devil? Hence, I say, let it alone. Dr. Collins proceeds to show that "the quantity of calomel and ipecacuanha taken in this way, in many instances, was very great; to the amount of three, four, or five hundred grains, or upwards. With some, in order to hasten its effects on the system, friction with strong mercurial ointment was diligently employed, and blistered surfaces dressed with the same * * in *several* instances, a scruple of calomel was given every second or third hour, and carried to a great extent; one patient took more than an ounce. I could not observe any better effect from the large doses than the small: the system was not more speedily influenced; and when they did so act, it was often with violence, so as to endanger the destruction of the soft parts about the palate." Why, then, give such a medicine? The woman is ill enough already.

540. The patient, No. 102, p. 456, took 520 grains of calomel. She seems to me to have had the disease while in labor, and died on the fourth day. Of the women who recovered, some took (as No. IX., p. 421) 264 grains, and No. VII., p. 417, "308 grains of calomel in the course of little more than forty-eight hours, in the early period of the attack."

541. I deemed it best to cite for you this high authority of Dr. Robert Collins, and I am sure you will feel as much obliged as I am, by his clear and candid instruction: yet, I presume, at the same time, that the effect of the excerpt upon your minds will be to excite a degree of distrust in the powers of mercurials, if not so great as Dr. Collins's, or as my own, yet some degree of

distrust and hesitation; and that you will, in fine, pause and decide within your inner self what the precise purposes may be, that shall lead you to write the prescription for calomel in the cases. I shall, perhaps, on future occasions, prescribe calomel for my patients in childbed fever, but when I do so, it will be mainly in view of securing its action upon the peristaltic muscles—one that I can comprehend; for I have a continual dread of that rest, or that sort of paralytic debility or inaction that seizes upon them whenever the peritoneum becomes inflamed. I have, in section 279, explained the grounds of this fear of injury, and also in section 278 the nature of that fatal angulation of the intestine with which she is menaced whenever the tympany becomes enormously great in a lying-in patient. Here, I say, I might prefer the calomel for its aperient and cathartic power.

542. Again: in the progress of a childbed fever case, when the inflammation appears to have come to a stand-still, or to make no progress in expansion or intensity, I might gladly avail me of the medicinal qualities of calomel, to combine with it, a portion of kermes and nitrate of potash or camphor, or all of these together with a view to their united influence upon the heart's rate, as well as upon the cutaneous excretions; an influence of whose existence and salutary nature I cannot for one moment doubt.

543. Perhaps, too, under certain circumstances, I might be happy to note the first signs of a ptyalism—out of an empirical, scarcely out of a rational inference that a ptyalism must supersede a moderate metritis or peritonitis. In the latter case, however, I may say that I should suspect the woman who can be cured by means of a salivation, could more safely, conveniently, and certainly be cured without it; for I regard it as one of the most improbable of things that a salivation, in our cases, could be brought on until a large moiety of the danger should first have passed away. It would, of a surety, be an extremely difficult task to produce a salivation while the phlegmasia should be upon the ascending grade. Many cases are on the record, however, of profuse ptyalism occurring during the course of treatment.

544. Among others, see Mr. Wood's case, by Bradley, in the Somersetshire Epidemic, Aug. 1811. I find the case in Ozanam (*Epidemics*, vol. ii. p. 290), and Baudelocque (*Fièvres Puerperales*,

p. 422) says he has noticed, in practice, many very severe salivations attended with considerable swelling of the tongue, gums, and cheeks—the parts being covered with aphthæ, and with exceeding painful ulcers. I have myself seen very bad salivations in in-lying women, but I repeat that such an incident is not to be wished for.

545. It is contended that mercurials do exert a great force of counteraction against what is denominated the inflammatory diathesis; and that the fibrinous element of the blood, too largely developed in inflammations, is lessened quantitatively by its use. The justness of this sentiment admits of questioning; for is it not true that a person, being bled, and exhibiting no siziness of the blood, but who takes soon afterwards mercury, so as to bring on a salivation, does immediately thereupon show what is called buffy or sizy blood, or inflamed blood? I believe it is invariably so; but were it even not so, we ought to remember that time is short and art long, and that Gordon quotes Horace's words, *hæc momento cita mors venit aut victoria læta*; and this is very applicable to our contests with childbed fevers—so that, in the generality of cases, we have no leisure to wait for the tardy power of mercurials, except those connected with their cathartic powers. Many patients begin to die as soon as they are found to be attacked. Have I not already said that neither is pain nor constitutional disturbance awakened early in all cases of uterine phlebitis; and that the fatal lesion is often effected before the alarm is taken or given? In such cases, I repeat it, there is no leisure to wait for calomel.

546. For thousands of persons, calomel and mercurials induce a gastric distress of a nature indescribable. This qualm, or sickness is, however, attended, for the most part, with a softer beat of the arteries, and that I take to be equal to a lessened violence of the sanguine injection into the inflamed vascular areas; but it is at the same time highly irritating to the bowels, and on that very account, less to be esteemed than milder aperients or purgatives.

547. You cannot make out of the study of the Mercurials, which, as M. Renouard says, sect. 537, are specifics in syphilis, any specific properties of those drugs as to our childbed-fever phlegmasias. You can only appeal to them as perturbators, or shock-giving medicines, and to that perturbation, or shock-cure, as a cure by

chance—merely a cure by guess. If I might be allowed by you, to introduce a homely *but* strong figure: those that depend upon them, are people to *make a spoon or spoil a horn*—whenever they employ mercurials without a clear understanding of the objects in view, and the probability of attaining to the ends proposed. Do you intend to purge the patient? Then use calomel, in God's name, if you prefer it. Do you intend to lessen the hardness and volume of the pulses by means of the qualmish state superinduced by calomel? Calomel will do this for you, somewhat. But do not resort to the shock merely to see what the shock may haply produce.

548. The majority of puerperal women go into labor with overloaded bowels, as I have said in sec. 344. This oppressed state of the alimentary tube is the cause of irritation that might well suffice, in epidemic times, to invite the affluxions of an incurable inflammation. Seeing, then, the strong empirical reliance to be placed in calomel as a purge, I consider it very discreet and prudent, in such a case, to exhibit calomel in the dose of ten or fifteen grains, after having premised the indispensable venesection; and, to insure a cathartic operation after calomel, where the meteorism is not already great, few things are preferable to Chaussier's mixture of syrup of rhubarb and castor-oil; two ounces of the syrup, with one of the oil, gives a mixture, of which half a fluidounce is a proper dose; to be repeated every hour or two hours until the bowels are freely enough moved. It appears to me that a dose of ten grains is a proper medium in the dosing with calomel; though, I believe, it is a matter of indifference nearly, whether we choose to exhibit six, ten, or sixteen grains for the dose; the effect being about the same for each of those quantities; such, at least, is the forced conviction produced in me by many years of observation. I may here refer you to what was on this point by Dr. Collins, in my sec. 539.

549. I cannot then advise you to repeat large doses of calomel every day. You should dread their power to irritate the bowels. Your patient has already irritation enough and more than enough, and it is by no means agreed, on all hands, that the influence of calomel on inflammation of the womb is so favorable as is by many supposed. Dr. Jörg, p. 667, observes: "Since the period

when calomel became with physicians so favorite a medicine, it arose into a custom to prescribe it in cases of this weakness (tending to putrescency) of the womb, without reflecting that, under these circumstances, it is not a resolution or discussing of inflammation that is indicated, but an arousing of the muscular action of the organ. Such administrations have, therefore, been productive only of harm. Calomel, even in the smallest doses, when given to lying-in women, does not lose its effect on the alimentary canal. It adds in this way to the uterine irritation, and thus carries the greatest disorder into the puerperal functions." Undoubtedly, where calomel does produce salivation, the inflammatory element in the blood, I mean the size, is at the same time greatly augmented in quantity, and though we rarely find that mercury has power to develop a salivation during the active stages of puerperal inflammation, we ought to suppose that it often approaches to that state, and so, that it often renders the blood more sizzly or inflammatory, instead of less so, as is by most American practitioners falsely believed.

I never could get a clear idea of the meaning of the technical word *alterative*. I often see it, and daily hear it pronounced, for it is in every man's mouth, and thousands of us, when asked the question, Why do you give the calomel? reply, We give it as an alterative. An alterative to alter what? If to alter the blood, we shall alter it for the worse—if it be true, as it is true, that whenever mercury *touches* the gums, the blood becomes at once buffy or sizzly, or inflamed, to use the common expression.

Your success will generally turn on the promptitude of your action. If you commence early and aright, you will have a success to console you, but in all these examples, you will have cured the disease, so to speak, at once. I do not mean here to say that all indisposition is at once to disappear: far from it. The shock the nervous and sanguine apparatus have endured by some eight or sixteen hours of childbed fever, may have so perverted the blood's crisis, so impressed the *membrana vasorum communis* (the *endangium*), so disturbed the nervous mass, that many days must elapse before the innervations shall become natural again. Some fever will remain; some local turgescence; some rebellious engorgements; and the sullenly retiring inflammation that you have conquered, but not wholly destroyed, may yet demand at

your hands further proper remedies. In such circumstances, I should not in consultation practice, object to a formula like this:

R. Calomel gr. vi.
Sulphur. antimon. aurant. gr. xii.
Potass. nitrat. gr. xxx.
Camphor xviii.

M. ft. sec. art. pulv. in chartul. No. vi. dividend. sig.

One of the powders mixed in syrup of gum, for the dose; to be repeated every second hour, or every third hour, *pro re natâ*.

550. In exhibiting such a drug, I should look to the moderating influence of the qualms produced by it, upon the force and volume and frequency of the pulse; to the cutaneous transpiration and the excretion of sebaceous matter and carbonic acid from the skin; while I should also, with some confidence, expect that under its operation, the mucous, bilious, and pancreatic, as well as urinary secretions, should be somewhat increased, with a positively ameliorated circulation in the meseraic, hepatic, and emulgent system of vessels.

551. Baudelocque, *op. cit.* p. 404, speaks of mercurials as part of the treatment for puerperal fevers; at 411, he attributes the glory (*la gloire*) of having proposed and exhibited mercurials as remedy in chief, to Dr. Vandenzande, of the Civil Hospital of Antwerp. I regard such a one as a very small glory indeed, or rather as only a *gloriole*; for if it could be established universally I am very sure it would not enure to the benefit of society, for whose good we exist, as a body, authorized by the commonwealth, to heal the sick with remedies according to our consciences.

552. I believe there are a great many of our brethren who confidently think that to touch the mouth—that is, to provoke a state of ptyalism—is among the most certain methods of cure. But I apprehend that as a general thing, the best informed members of the profession will not, under ordinary indication, seek to bring on salivation in our cases—the violent disorders of the constitution making it always unlikely that a salivation can be brought about until the most dangerous stages are past. As to a salivation effected with a design to rescue the patient from danger, in some of those very protracted and complicated disorders

that women fall into in the course of certain attacks of childbed fever, I shall only say that I have no doubt it might, in very rare instances, be deemed a useful and found even a laudable practice. Yet, after all, the poisoning by mercury is so dreadful a thing, when the use of mercurials is pushed to that extent, that a Christian and charitable man would, it seems to me, pause long before resolving on the course.

553. I have only further to notice the use of mercurials as a remedy in our disease, by inunction of the unguent. hydrarg. I have witnessed this mode of treatment at various times for more than thirty years past, and have attended patients for whom the application has been prescribed in consultation. I can very honestly aver that the only sensible effect I have ever observed from the method was, that it produced in me a feeling of utter disgust, and never did any good for the patients; and certainly, nothing could be more disgusting than to see the thighs or abdomen of a sick woman all begrimed and stained with that detestable compound of grease and quicksilver—a belly scarred with leech-bites or cup-marks, raw with a recent blister, and then befouled with black mercurial ointment, is a thing not to be conceived of for a lady, or for any other woman. If you will shock—if you will salivate, I pray you do so with calomel or Van Swieten's liquor; but do not, I beseech you, employ mercurial ointment, it is too nasty; and this is all I shall say on the subject.

C. D. M.

LETTER XXIII.

PURGATIVES IN THE TREATMENT OF CHILD BED FEVERS.

554. YOU cannot get along, in the management of childbed fevers, without the use of remedies calculated to maintain the peristaltic power of the bowels; and this thought brings me to the consideration of purgatives as a treatment in part or in chief—for, you must know, that there have been persons disposed to rely on cathartics as the remedy in chief for the cases. To this effect, examine the early cases that fell under the younger Hey's conduct in the sad epidemic at Leeds, and observe how fatal was the malady until he became suddenly enlightened and reformed by the revelation from Dr. Gordon's work, which happily came into his hands. Indeed, Gordon himself fell at first into the same miserable error.

555. It were well for you here to make the reflection, that the ordinary contents of the bowels are normal contents, and that their presence in due amount is an essential condition of health and comfort. Any excess becomes productive of disquiet or disorder. But this applies only to the state of health. When diseases assail, and especially violent febrile diseases, then the contents of the bowels, though they be normal in the healthy individual, become absolute abnormalities, for the vital state of the intestine is changed, and so the normal relation of it to its contents is destroyed. Hence, all mankind know that in fevers it is useful to cleanse the bowels of their contents, and afterwards furnish them with materials suitable to their new and diseased vital condition. I say, then, that in febrile diseases, methods of producing evacuations are indispensable to a successful treatment.

556. I believe there is a considerable difference in the opinions of medical people on the question of purgative doses in child-

bed fever; and although upon the naked proposition or question, are we to rely upon cathartic medicines in the case, I should promptly answer in the negative, yet I repeat, you cannot get on without the use of them. But they should be employed understandingly, and not with a blind rage to cure by evacuations alone. They cannot cure the patient by what is properly called catharsis—that is, by causing such abundant excretions from the mucous and other secreting apparatus of the intestine as to effect a complete disgorgement of the inflamed capillaries, and so bring the case to a close by resolution.

557. I consider it behooves every accoucheur to be not a little chary of his purgative medicines as to all women recently delivered; and whenever, from observation, he is led to suspect the approach, or the actual existence of an epidemic of childbed fever, he should use them, at the beginning, only with the greatest circumspection and reserve. A physician always has reason to suspect an epidemical influence, when he finds it unusually difficult to get his patients out of bed; for while, under the ordinary circumstances of practice, he finds it a difficult task to keep them in bed long enough, it does sometimes happen that in patient after patient, he is vexed with the delay to get up and go about; and that too, without any perceptible inflammation or any strong fevers, but from indisposition so vague as scarcely to be described. Dr. Joseph Clarke found this to be the case in his hospital practice, and I believe you will find it so likewise, in general practice—at least, I have made the observation again and again during my medical life. Dr. Joseph Clarke, *Observations on the Puerperal Fever*, &c., Syd. edit., p. 355, says: “It was a general observation, that our patients recovered slowly, or, to use the language of the nurses, it was much more difficult to get them out of bed than usual.” Dr. Clarke is here speaking of the health of the hospital previous to the breaking out of the epidemic there in 1787.

558. I say, then, I consider that the accoucheur should be a little, and sometimes very chary in the use of purgative medicines for women in the first days of a lying-in, because the operation of a smart purge, which is attended with griping pains and intense muscular activity of the bowel, is not rarely the first stage

of an irritation that shall end in forming an area of phlogosis that must hasten to end only in death. I am most confident that I have repeatedly seen women suddenly attacked with peritonitis or metro-peritonitis, solely because they had been imprudently and intempestively dosed with purgative medicines. Hence, while I am invariably inclined to be very watchful against costiveness in the *accouchée*, I am as habitually disinclined to procure the first motions by means of strong medicines; and in the majority of my cases, prefer to cause the required dejections by means of the simplest enemata, or mildest aperients, as of pure water, or water animated with a little salt, or with Spanish soap, or at most, a dose of oil, or magnesia; and I now earnestly advise you, especially when the season is a suspicious one, to follow my example. What the woman requires is, to have the bowels properly opened, once, and not in general more than twice. One can hardly fail to get at this effect by means of a water-injection. The woman is not a sick woman, in the sense of being sick, she is only a woman confined, and liable to sickness. The bowels will not move spontaneously; let them, then, be moved by enema only, since she has no need of a cathartic. It is time enough to give the enema on the third day, as a general rule; and the aperient dose on the sixth or seventh days; if, indeed, she should at all require such a prescription; a thing probable, owing to the almost uniform costive habit of body in the latter weeks of uterogestation, as I have explained in my sects. 244-247.

559. But, as we are considering in this Letter, of the use of cathartics as proper remedies in childbed fevers, I shall here beg you to discriminate betwixt the cases of metritis and metro-phlebitis, and those of pure and simple peritonitis. In the two former, without concurrence of the latter, we have not to contend against those terrible distensions caused by tympanitis; for the belly may be soft and supple, instead of enlarged beyond the volume of the abdomen at term. It is against this frightful tympanitic distension, that the use of purgatives should be directed. Tympanitis is over-distension of the bowels, and nothing else; and I see not how any one could reasonably expect to conduct to a term, in resolution, a tympanitic case of puerperal peritonitis without the use of aperients or purgatives. Indeed, the inconveniences and dangers of that state are so alarming, that I can con-

sider no other part of the case more so. When the distension reaches a certain magnitude, the gut threatens to shut by its angulation, at the return, as effectually as if it were tied with a surgeon's ligature, or fatally closed by the strangulating edge of Gimbernat's ligament, or the ring of the abdominal canal.

560. A bowel inflamed in a lying-in woman, always yields to the lateral pressure of the gases extricated within its tube; and it is far less a matter of moment to purge forth the remains of the digestions, the bile, the mucus, &c., than to expel the gases that, by distending them, not only prevent resolution, but, by adding unspeakably to the pain and constitutional irritation, exert the most noxious influence on the aeration of the blood, and the respiratory power in general. A woman with an inflamed belly as big as that of a woman with twins, and more sonorous than a tight drum-head, cannot possibly breathe aright; she must take two breaths for one; and even at that rate, procure less oxygen than she requires; so the blood turns slowly, less oxygenated. Her tint grows paler or livid; and at last, a good share in her death is attributable to the sort of cyanosis thus superinduced. Yet this sort of half-way, half-suspected, half-seen asphyxiation in childbed fevers is a strong thick bar against her recovery-power. How can the neurosity that is essential to the life of the organs and the play of the functions, be extricated, when the respiration-power, like a steam piston cut off at half stroke, checks the descent of the diaphragm in the respiratory cylinder of the chest. I implore you to be watchful here, and employ the best means in your power, to obviate the direful tendency to tympanitis in all the peritoneal childbed fevers; for a great tympanitis is equivalent to a great cyanosis for the dying woman.

561. The alarm I always take at such occurrences, and the impending danger to the woman, makes me willing, in the commencement, to seek for the powerful cathartic qualities of a strong dose of calomel; and therefore, I should willingly prescribe a dose of ten grains—though uncertain whether five or six grains, or sixteen grains, might not be as well for her. Yet I should be unwilling to confide in the mercurial alone, and, therefore, should administer a secondary dose, consisting of castor-oil, magnesia, senna, or a combination of syrup of rhubarb with castor-oil.

562. Provided the alvine evacuations from the calomel and its adjuvant should prove to be stercoraceous, and in such quantity as to justify an opinion that the intestinal canal was cleared of its ordinary contents, I should forego for many succeeding days, any further intention of causing purging in the ordinary sense of the term, and taking due care as to the ingesta to be allowed during the continuance of the disease, I should, for the rest, content me as to the duty of moving the bowels, with relying upon enemata, of a simple, and generally the least irritating kind.

563. Nevertheless, there is constant need to obviate the tendency to distension from meteorism; and, therefore, there is need sometimes for aperient doses. My master, who inducted me into Medicine in my early student life, was accustomed, in cases of tympanitis, greatly to rely upon a combination which he derived from his own instructor, Dr. Brown, of Maryland. I was in the habit of frequently preparing the doses for my master's patients, as well as of witnessing its effects in the subduction of the symptom. This was so long ago as 1809 and 1810—now forty-five years ago. I have repeated Dr. Brown's method many times during this long period, and now advise you to resort to it when the proper occasion may present itself in your own practice. The medicine is prepared by pouring eight ounces of boiling water upon an ounce of manna and a drachm of anise seeds. After the mixture has rested for half an hour it should be strained, and the cold infusion then added slowly to three drachms or four drachms of carbonate of magnesia, in a wedgewood mortar, taking care to reduce the mixture to a state of complete smoothness, or freedom from grains.

564. The dose is a wineglassful or less; to be repeated once in an hour, or two hours, until it excites the action of the bowels. I confess, I do not know a better carminative dose than the above, and time has not altered my conviction that it is a most safe, convenient, and efficacious medicine for such purposes as I have above indicated.

565. I may here mention that I have found doses of tincture of *nux vomica* to act very fortunately in reducing the tympanitic distension, in certain cases of childbed fever. The doses, say five drops every two or every four hours, may be safely exhibited.

566. There is further a mixture much used here that was a favorite prescription of our beloved Dr. Ph. S. Physick, Professor in the University of Pennsylvania, which consisted of:—

Carbonate of potash ʒj;
Gum-acacia ʒj;
Tinct. of opium 40 drops;
Water 6 ounces;

To make a proper mixture. The dose is a tablespoonful, which may be repeated from hour to hour. Sometimes an aromatic distilled water, as spearmint or cinnamon water, is used in place of the pure water. I think you will find, upon trial, this mixture of Dr. Physick is a convenient and useful medicine in the cases. De la Roche, p. 213, tells of a case of frightful tympany that occurred early in his practice at Geneva. He saw the woman the morning after her delivery in a bad labor by the forceps. She was in agonizing pain, and excessively weak; with the abdomen so greatly distended as to be much beyond what is usual at the ninth month of pregnancy. It was excessively sore, and intolerant of pressure; the pulse small and frequent. He says that, at that period, he was unacquainted with puerperal fever; though, since that time, he wrote, as I should think, one of the very best accounts of it that has ever appeared in the medical library. The weather (July 1) was very hot, and her apartment particularly so, which filled him with apprehensions of putridity; wherefore, to give renewed activity to the muscular apparatus of the intestine, he made powders of 12 grains cinchona, and 5 or 6 grains of rhubarb, each one of which must be given, for the dose, every third hour. The medicine brought on a gentle purging, without pain, and considerably reduced the volume of the belly. He next substituted a decoction, of one ounce of cinchona, to be taken in the day, and repeated it several times, without recourse to any other essential remedy; the alarming symptoms were dissipated, and the patient found herself, shortly, cured. I thought it might be interesting, and, perhaps, at some future time useful to you to read this account of De la Roche's conduct in the case; but I should warn you that he was a believer in the supremacy of venesection, as much as I am. But that is no reason why his well-conceived combination should not

have rescued the woman, whom he would most certainly have bled, had he attended her at a later period, and when he had become better informed.

567. As a useful and mild aperient, it is convenient to direct a pill composed of 2 grains rhubarb, 1 grain extract hyosciam., and 1 grain extract gentian. Two such pills, repeated every three or four hours, are likely to excite the peristaltic fibres to such a degree as to enable them to expel the distending gases of the bowels. In like manner, a powder of 5 grains rhubarb and one of calomel, repeated every four hours, is a probable means of effecting the same purpose, which is not to purge the bowels, but to get rid of the gas of the tympany.

568. Enemata often succeed better than aperients or cathartics. A pint of warm water, with half an ounce of salt, or with 2 scruples of jalap, or with an ounce of Spanish soap, or with a tablespoonful of oil of turpentine, made by means of the white of an egg into an emulsion, and united with an ounce of castor-oil; any of these may answer the purposes sought to be obtained.

569. Where all these methods fail, a complete success occasionally follows the use of a large catheter introduced into the intestine. I am convinced that I rescued a woman in Lombard Street from an otherwise inevitable death with childbed fever, by this method. I procured a male catheter of the largest size, and fortunately, conducted it along the colic sigma, its whole length. The horrible tympanitis disappeared immediately, but recommenced upon the removal of the tube. I replaced it, and kept it in the bowel for more than twenty-four hours, so that all the gases she developed within the intestine passed freely through the tube of the catheter, and so, gave her no inconvenience. A person moribund from tympanitis, and completely insensible at the time, and nearly pulseless, was rescued by me from impending death, in the course of a few minutes, by the same means. It must be admitted, however, that in endeavoring to procure relief and safety for the sick by such a method one is frequently foiled, in consequence of the packing of the bowels, and the short, quick returns, amounting almost, and sometimes quite, to irremediable angulation of their tracts.

570. I have been much struck with the frequent accounts and

complaints, of the French writers in particular, as to diarrhœa, which seems to have been for them one of the most troublesome and unmanageable complications of childbed fevers. I do not conceive that the English writers have made the same complaint to a similar extent; and, as to my personal experience, it is null as to this point, though I am sure, having long been aware that it was regarded in Europe as a common and very bad complication, I could not have ignored it, had it been common here.

Legouais gives us a dissertation upon this diarrhœa, in arguing the question as to the propriety of using purgatives, at page 55, where he speaks of it as "*cet accident si redouté dans cette circonstance.*" Legouais appears to me to have been led into his fondness for purgative doses in childbed fever cases, by his terror at what he denominates, page 53, "*ce symptôme effrayant de la constipation,*" and well he might; for there can be no more frightful condition for a perishing woman than that, which, by the way, Legouais did not at all comprehend, because he never caught the least idea of that angulation of the bowel which I have so often, in these Letters, spoken of, and which is the cause, uniformly, of the constipation that excited his apprehension.

571. I am happy to find that my own experience and personal convictions concerning the use of purgative medicines in our disease coincide with those of Legouais, who advises that they should be employed rarely, if at all, and that the after-use of evacuants should be restricted to a choice among the gentler aperient drugs.

572. It appears to me unnecessary to present any further remarks or suggestions on this particular topic. I now close this Letter, with assurances of that regard with which I always rest
your friend and servant,

C. D. M.

LETTER XXIV.

OPIUM AND TURPENTINE-OIL IN CHILDBED FEVERS.

573. How should a physician think, *tuto cito ac jucunde*, that is, safely, speedily, and gently, to conduct a case of childbed fever to its close, without the assistance to be derived from anæsthetic and anodyne medicines? There is too much irritation, too much pain, and too much restlessness and discomfort, to allow us to do so; and this is the reason why, in the management of the cases, almost all medical people do, with greater or less confidence, resort to the employment of some of the preparations of opium: opium has repeatedly, among physicians, been pronounced to be a *magnum Dei donum*, a special blessing given to us by the beneficent God. There is an eminent German philosopher who says: "The universe is not, in deed and truth, that which it seems to be to the uncultivated and natural sense of man; but it is something higher, which lies behind mere natural appearance. In its widest sense, this foundation of all appearance may be aptly named the Divine idea of the world." Professor Fichte, in the above sentences, only reiterates the sentiment of Plato, who saw, in all matter and form, nothing more than the material realizations of the Primary and Eternal Ideal—that which precedes all real and finite; itself being the principle, the force, and the mould upon, by, and through which, all things exist as nature. Nature, therefore, according to this philosophy, is fashioned and sustained in conformity with a Divine Idea of it, which precedes all form, all substance, and all force in nature. All things, therefore, having been created according to a pre-existent Divine Idea of them, things, or the real, are only representatives of the absolute, or the True Ideal. To know the real truly, it is necessary for us to attain to, and possess ourselves of the Divine Idea of the world.

Such are the opinions of the German author above named, and I have cited them here, only that I might use them to show you how grateful we should be, as ministers of nature, for the multiplied benefits it is our privilege to scatter broadcast among our fellow-creatures, of whom we find so many in sickness, in pain, and in fear.

574. It is from the Author of Nature—the giver of every good and perfect gift, that we derive our privilege to know his laws, his world of matter, his forces of things. It is to him that all things tend and turn, at last; and as HE is all-wise, we shall never become truly his servants and ministers in our great mission as Physicians, until we can seize, and hold the Divine idea, as to all that concerns our own ministry. Hence, as physiologists, we should inquire into the laws of life, as pure and simply expressions of his will; for as a sparrow falls not to the ground but through his agency, so no vital function—no medicinal power can be, for the twinkling of an eye, independent of his purpose and power. Every due performance of the functions of the body is consistent with its health, while every aberration from the law of its normal life, is infraction, and like moral turpitude and sin, brings with it the inevitable consequences, sickness or death, as infractions of the moral law draw after them remorse, with sorrow and shame. Consider how good was God to give to mankind the sleep-giving poppy! The plant existed before the pain! The Divine idea preceded the form and the substance: yet it was fashioned and moulded, and endowed with its powers, because pain must be. It was never an accident—it was already become, while pain was yet becoming. The idea of it existed before its root was set in the soil, or its germ was formed within the secret chamber of the ovary. It was in conformity with the pre-existent idea of it, that those strange, mysterious chemical powers were bestowed that stored up its sanative juices in its capsules, and converted them into opium; for it was *He* who saw those material pains we could not wholly avoid, and *He* who foreordained for us the “sweet oblivious antidote.” I cannot think of the gift of opium to mankind, without finding in it my warrant to become a physician, nor without feeling, at the same time, the spur of conscience that pricks every rational being on, and still on, in the course of duty—duty to learn daily more and still more concerning the Divine

idea of the universe—duty to search after, and to the utmost of ability comprehend the nature and laws of the animal economy in its health and its diseases, with the rules of its subjection to the powers of medicines.

575. All medicines are the gift of God, and all true physicians are ministers for those gifts, worthily to apply them, with wisdom and virtue, for the obviation and the consolation of many inevitable ills that attend the physical and moral nature of man.

576. The cereals, the leguminous vegetables, the fruits, and the grasses, are severally endowed with powers and with qualities, designed and adapted to fulfil ends foreknown in the Divine Idea of them. We are all commanded daily to express our gratitude, as well as our dependency for our “daily bread,” and there is no wise man can look out upon the scene of the world without a feeling of gratitude to the Beneficent Disposer of all. Yet, among a thousand blessings, what is there more strangely good, more curiously kind, than those medicinal things that we bring to the wounded and the sick! We have not, it is true, the direct, energetic, voluntary force of Christ and the Apostles, to say unto men: *Θέλεις ὑγιῆς γενέσθαι; Ἐγείραι, καὶ περιπάτει;* but we have discerned a thousand means by which to bring about results almost as miraculous. How studious, then, should every man of us become to attain to a clear and plenary comprehension of the nature and power of the substances employed as remedies in the practice of Medicine; to go behind them, and discover and possess the Divine Idea of them! Considering the peculiar relations to society, and more than that, the intensity of the interests of family and offspring, concerned in those dreadful and dangerous circumstances it is the object of these Letters to explain, we have abundant reasons to felicitate ourselves upon the possession both of the material and the knowledge, which puts it in our power, by means of the various preparations of opium, sometimes to withdraw our friends out of the dark and difficult ways of disease and pain into the repose and happiness of dangers past, or health and pleasures regained with the life we have saved!

577. I can readily conceive of cases of childbed fever that should be curable by opium alone, and unaided by any other means of treatment; for I can suppose, that in certain epidemic

constitutions of the atmosphere, injurious influences, exerted by the epidemy upon the nervous system, may and do develop the phlegmasias of childbed fever, where those influences are allowed to go forth unchecked to work mischief in the subject tissues. Even when this influence has gone so far as to cause the earliest areas of inflammation to be laid, a powerful dose of opium might suffice to recall the wild energy, or the confused disorder of the nerves of the part diseased, back to an habitual obedience, and recompose the harmony of the disturbed organisms.

578. Many a painful or molesting sensation, that was interpreted as nervous or spasmodic, and that was quelled by anodynes, was, perhaps, due to the incipient stages of a phlegmasia, which, but for a happy conservative intervention should have progressed to the open, unmistakable, and mortal phases of a childbed fever. I conceive that the supposed after-pains so often checked and dispelled by our prescriptions of camphor, or of opium, are often, in reality, only the beginnings of these mortal phlegmasias, and, therefore, I say that opium is capable of curing them, without extrinsic aid.

579. There are physicians who have been, and, perhaps, are still willing to heal all their childbed-fever cases with enormous doses of opium alone—making use of no other medicines, at least, during the violence of the disorder. And too many facts are before us to allow us to doubt that successful results have, in this way, been obtained. I have been informed by a physician in a neighboring town, that, in his treatment by opium, he has carried the narcotization so far as to keep the patient's respiration, for hours, as low as five or six breaths per minute; and I have heard of its being reduced so low as four respirations per minute, the treatment being followed by the escape of the woman from death from both causes, the phlegmasia and the drug. Any successes obtained in this way, ought not, in my opinion, to be regarded as triumphs of our art, but rather as misfortunes, that serve to retard its progress in usefulness and in the confidence of the people. Even though one, or twenty persons should recover from childbed fever, after such a poisoning by opium, no man can expect that the method shall ever receive the general sanction of the sober portion of our class; and there is danger, in the mean time,

of the adoption of that way of cure by many silly young people, and of the greatest mischiefs to the sick, following their imprudence. Nothing but the greatest familiarity with the operation of opium, and the most precise knowledge of the physiology of man, should warrant, if even they could warrant, so extravagant an employment of the poisonous power of the drug.

580. From all that I have heretofore said in these Letters on the subject of childbed fever inflammations, and of bloodletting as the chief, if not sole reliable remedy for them, you could not suppose that I should advise the use of opium in the first hours of the treatment. Far from doing so, I consider that you ought to regard the most important ends of all treatment to be in a good way of being secured, by first impressing upon the phlogosed areas a disposition, and affording them a power to recover by resolution, not of the membranous phlegmasia only, but even the phlegmonous nucleus of it, if any there should be. When this first and all-important step has been taken, the opium comes in most fortunately as an adjuvant, and it may be exhibited in combination with calomel or tartar-emetic before the time has arrived to confide in its force pure and simple. If, therefore, you should deem it advisable for the woman to be placed under the influence of calomel or an antimonial, I advise that opium be conjoined, and that, in a very positive dose, such as one to three or five grains. I should recommend the omission of the opium, in a case where it might be urgent to secure the speedy evacuation of the bowels, in which case calomel is not the most desirable article. Probably, a majority of our physicians do prefer to conjoin the opium with the calomel, to give which, alone or uncombined, is a thing, I believe, rarely done; it being in fact the custom to exhibit, at some moderate interval after the mercurial, an aperient or cathartic medicine, which, being designed, no objection can arise against the administration of opium along with the mild chloride of mercury at the beginning.

581. In all the subsequent treatment, the woman should have the benefit of that tranquillizing power attributed to opium. By its means she may be in a considerable degree protected against those reflex influences, which, in their great irritation, the nerve-centres are likely to throw back and expend upon the various organs;

thus spreading the mischiefs far and wide throughout their centro-peripheral domain. To procure this protecting influence, the doses of the opium should be full ones, and, I may here suggest, that the consumption of the drug should amount to from four to six grains per diem, when a chief reliance is placed upon it.

582. The opium may be employed as pills, as laudanum or black drop, or as morphia; or the state of the skin and the primæ viæ may indicate the use of Dover's powder, in preference to any other formula. Dover's powder, made into pills of four grains each, and repeated every second to every fourth hour, does, in many of the cases, answer our reasonable expectations. In this way it may be continued *pro re natâ* indefinitely. The dose rarely provokes nausea, while it promotes a highly desirable diaphoresis.

The *Amer. Journ. of the Med. and Phys. Sciences*, edited by Dr. Isaac Hays, Jan. 1852, contains, Art. XI.: "Case of Puerperal Fever treated successfully with Opium in large doses. By Wm. Kelly, M. D., Resident-Physician to Blackwell's Island Hospital, New York City." The patient, æt. 24, primipara, was delivered Dec. 14, in the morning, with a labor of thirty-six hours. No case of puerperal fever had been in the house for months, though two cases of erysipelas were, at the time, in an adjoining ward, and had had the attention of the author. At 4 A. M., 15th, less than twenty four hours, the woman had a severe rigor. Dr. R. saw her at 5 o'clock. The face anxious; deep sighing; pulse 120, small and hard; pain and tenderness in right iliac region in space as large as a palm; no lochia; milk not yet come.

Dr. K. gave a teaspoonful of laudanum, which was repeated in twenty minutes. Afterwards, three grains pulv. opii ordered every half hour until asleep. At 7 P. M. pulse 150, quick, hard, small; pupil contracted; sleeps, but not profoundly; easily roused; whole body bathed in perspiration; tympanitic and tender in right iliac region; a blister 12 by 12 on belly; 12 P. M. no change of condition; has taken sixty grains of opium; soon after 12, vomiting, which probably threw off most of the opium. Dec. 16, 6 A. M. pulse 120, fuller and softer; less sighing; no lochia; breasts painful; vomiting continues; no more tympanitis; vesications from blister. Two grains morphia sprinkled on the blistered surface every two hours. At night, one grain morphia in

solution every two hours until asleep. By a mistake she got, by morning, six grains morphia, and had ten grains on the abdomen. At seven, somewhat narcotized; pulse 120; breathes seven per minute, with gasping; pupil a point; yet she did not sleep; heard all in the ward; replied to remarks not addressed to her. Sinapisms and Granville's lotion to back and chest; respiration normal; pulse 140; cold water to the head. 17th, 2 A. M., pulse 140; sleeps lightly. At 3 o'clock, three grains morphia. At 4 o'clock, respiration seven in the minute. At 8 o'clock, pulse 125; sweating most profuse; no pain. Two alvine dejections yesterday; urine free; face flushed and pinched; morphia one grain every two hours. During this day, pulse 120. 18th, morphia continued at same rate; bowels and bladder freely evacuated. 19th, she says she is "first-rate;" three dejections to-day and three yesterday. 20th, six dejections yesterday, soup-like consistency and pea-green in color; lochia came on in evening. 23d, pulse 100; has a "bed sore;" cannot sleep without morphia; milk in breasts. 27th, well, lacking strength; bed-sore; morphia in lessening doses. Jan. 1851, milk not re-established fully; child given to a wet-nurse; otherwise the woman "seems to have quite recovered from her sickness." "Within nine days" of the attack she was well. In this time had taken ten grains morphia applied to a blistered surface, eighty-four by the mouth, besides seventy grains of opium. For six days she took twelve grains of morphia daily; was seen every hour either by Dr. Kelly or Dr. Jenkins: The medicines were of good quality. The treatment, he says, "produced semi-narcotism for twelve hours. No function seemed to be injuriously affected by these large doses. Indeed, the bowels were kept in a relaxed condition. The kidneys performed their office as usual." Such is the very interesting account given us by Dr. Kelly, one which, I should think, you will examine with much pleasure, because you will learn from it that an adult has taken so very large a quantity of opium and morphia, without being destroyed by them. Computing that eighty-four grains of morphia are equal in force to 13,440 drops of laudanum, and that seventy grains of opium are equal to 1,440 drops of laudanum, we may conclude that the opiates used were equal to 14,840 drops of common laudanum, all taken in the course of nine days, which is equal to 1,648 drops of laudanum per diem during nine con-

secutive days. You will see, from the perusal of this statement, that Dr. Kelly cured his patient, and you will also see that the opium practice, though fortunate in its result in this instance, is not one that can be recommended as a method to be always, or even often taken, but only in the very worst and most exceptional instances.

583. Seeing that the chief seat of the mischief, in certain of the forms, is within the pelvis; and further, reflecting that opium doses are not much less active when used as enemata, we have many inducements to bring about the anæsthetic effects of the drug by anodyne enemata; and when the circumstances do not oppose it, I presume it may be more convenient, as well as more useful, to employ the injection. In my opinion, your anodyne enemata should never contain more than forty-five drops of laudanum for each dose. This, to many of you, will appear to be a very feeble practice; but I hope you will not consider it so upon second thought. Indeed, I now earnestly advise you to set it as an invariable rule, as long as you shall continue in the practice of Medicine, never to go beyond this quantity. Otherwise, you will be exposed to the risk of committing a gross error in your dosings; for some persons are of a nature to be dangerously narcotized by the imprudent teaspoonful doses of laudanum that are commonly, in the United States, used in each enema. If you say that forty-five drops is too small a dose, you have the power to reiterate it until the desired effect is produced; but if you give an overdose, you cannot readily repair the mischief you have done. This habit I have had for more than twenty years, having engaged in it at the earnest solicitation and protest of our good and wise Dr. Physick, who told me that he had not broken the same rule, he had formed for his own guidance fifty years before.

584. The anodyne enemata are designed to procure both diminution of pain and sleep. I should think, therefore, you would provide for the repetition of the medicine, *pro re natâ*; that is, as long as the pain and vigils seem to require.

585. Many years ago, when I was young in the profession, I was accustomed to hear much and favorably of Brennan's remedy for childbed fever; for the celebrity that writer had obtained in Dublin, had wafted his name to this side of the Atlantic. I saw

his method tried in vain. Still, as years lapsed onwards, the name of Brenan and his method was often echoed, and many a sick woman was subjected to the Brenan treatment. This Dr. Brenan, who rose into notoriety by giving oil of turpentine to cure childbed fever, appears to me to have been a person of very imperfect education and worse temper, as any one would opine upon reading his pamphlet, now lying open before me. This pamphlet is, *Thoughts on Puerperal Fever, and its Cure by Spirits of Turpentine; Illustrated by Cases in the Lying-in Hospital, Dublin. Also, Cases of Inflammation and Spasm cured by the internal and external Use of that Medicine, with Correspondence on the subject:* by John Brenan, M. D. London, 1814; 8vo., pp. 24.

586. Dr. Brenan commences by referring to the confusion of opinions concerning the disorder, its terrible nature, and the failure of the methods of cure, both in private practice, and in the great establishments at Paris, Edinburgh, and London, as well as Dublin. He speaks of the fatal epidemic in Dublin Hospital, of 1812, when "whole wards were swept away"—of the intervention of many of the city physicians, who humanely proffered their assistance, which was in vain—of the bleedings that were tried, and the blisters and purgations, with all which, "the malady was no way arrested." p. 6. He next proceeds to tell us that about this period he was called to see private patients with bilious fever, whom he was much astonished to find laboring under "all the symptoms that he (I) saw in the patients laboring under fever in the Lying-in Hospital." He "generally found that, after even the most favorable labor, the patient sickened on the third or fourth day, with what nurses call the weed," but which proved to be fatal childbed fever.

587. Brenan says he had extensive practice among the lower classes, and that his reputation for success led to his being called in consultation by the ablest of the faculty. It struck him that by beginning early with evacuating the bowels, the weed might be prevented from going on into puerperal fever, which he deemed contagious, and that by a little calomel and antimony, it could be prevented from assuming the type of putrid fever. I shall not annoy you by citing his stupid 8th page, filled with idle hypotheses, worthy of a very lame first-course Student. You shall,

however, judge of him by his saying: "I should think that no woman has died of this complaint whose bowels have yielded to proper medicines;" as if such a thing commonly happens as that of a woman getting out of this world, and the hands of our profession, without having had such dejection. You know such a case would not happen except in those instances of what Legouais calls constipation, but which I have explained as depending upon angulation of the intestine. Omitting his paragraphs in succession, we come to page 11, where his words are: "My manner of healing this disease has been so marked with success as to cause much astonishment. The exhibition of the spirits of turpentine in a disease termed inflammatory, was not without the tribute of censure that useful novelty never fails to extort; however, its effects have borne it through (a sad mistake); and though theorists could not understand its mode of acting, and though some great men were not pleased that one not so great as themselves should make a discovery that revolutionizes medical practice, the application of it has met with success, and the good effects I have witnessed from it has consoled me for the displeasure that my success occasioned." You see already that Brennan had brought himself into conflict with the Dublin brethren, who doubtless had good reasons to distrust his temper, if not his clinical results.

588. Margaret Rogers had been bled twice, to the amount in all of thirty ounces. She was sitting up, vomiting green and yellow bile incessantly; could not bear the weight of a paper card on the belly. The hospital physician considered that effusion had already taken place, and as the woman was in a rapid state of dissolution, he desired to know what Brennan would give her. "I told him the spirits of turpentine. He started, and asked me how much; I told him a tablespoonful. This appeared to him a madness, but as the woman seemed dying, he said I might give a few drops, but that he would not stand by whilst I gave it."

He gave three teaspoonfuls of spirits of terebinth. at 2 P. M., in water. At five o'clock he returned, and the assistant said she was better, "and had the courage to stand by when I gave her a tablespoonful." "He asked her what she thought it was; she replied, Geneva water." "At nine o'clock, she lay at her ease." From the time she took the dose she never vomited; the belly

became flaccid, and insensible even to violent pressure. The next day, she had renewed pain, which vanished after more turpentine. Complained of hunger, and asked for food. She never complained afterwards of uneasiness in the region of the uterus, but in about four days began to spit pus, whereon her pulse sank and she died. Brennan "put her death down to the bleeding." The second case was that of Margaret Conolly, in hospital. Two days in labor, "and got the fever;" bled thirty ounces at two bleedings; belly tense and sore to an exquisite degree. Brennan poured the spirits of turpentine "on her abdomen, covered with flannel." In about three hours afterwards, she told him he had cured her; the abdomen flaccid as in health, and bearing any pressure; the breathing easy. "I gave her two teaspoonfuls of the turpentine in hot water and sugar," which disagreed with her stomach. Cold water he thinks a better vehicle. On the next day her symptoms returned, and she was bled eighteen ounces in the morning, and eighteen other ounces in the evening. The day after, she begged him to apply his remedy as before, which he did without permission, for humanity's sake. The belly became flaccid again, and could bear any pressure on it. She sank, but with no distress in the uterus, and after death the abdomen was flat. "I put down her death to the bleeding." P. 15.

His third case was that of * * *. Headache; sore abdomen; a burning in her stomach. Moaned much, and complained severely. "I gave her a tablespoonful of the *oleum terebinthinæ*, and a sup of water after it. In about fifteen minutes I came to her; she told me she got ease. On coming into the ward next morning, I missed her; she was sitting at the fire, very well."

In case four, the woman had symptoms, the usual forerunners of the disorder. Brennan "contrived by stealth to give an ounce of the turpentine in some saline mixture." All the pain and vomiting abated, and the consulting doctor said next day nothing was the matter with her. She went home that day, and, the symptoms returning, she was visited by a hospital attendant, who blistered, bled, and gave her the usual remedies. She died, vomiting green bile, with her belly swelled. "In this woman I stopped all the symptoms."

Mrs. Murray, case five, confined on Saturday, was seized on the Tuesday following. Had cough, with severe pain. Brennan applied

turpentine to the abdomen, and gave her a tablespoonful in sweetened water. She was next day free from pain, and ate bread and milk for breakfast. Then took cold milk, and got as ill as ever. Turpentine repeated as before. The next day, she ate "stir-about, and got a relapse from cold beer;" very ill till Monday, when the doctor showed her to Brennan as "a forlorn case." "He showed me the blackness of her hands, which he said was the sure forerunner of death, and that Denman said no woman ever recovered that had it." Another physician present said she was moribund. Took nothing this day (Monday). She was sitting at the fire vomiting green bile. "I gave her an ounce of the turpentine, repeated it in an hour, and applied it to her belly. The next morning I found her asleep." On Tuesday, he gave her castor-oil and tincture of senna, with two drachms of turpentine in the draught, which purged her much. Wednesday, breakfast of stir-about and milk. Thursday, *idem*. Friday, she sat all day at the fire, and slept at night with the next case. On Saturday, requested to be left in the hospital, but on Sunday walked home to Barrack-street, with the child in her arms. Brennan evidently looked on this case as a great triumph.

Bridget Cullen was the subject of case sixth. Had twins on Saturday, February 12, with hard labor. On Wednesday she was severely ill, with headache, and "turning of her stomach." Next day she had the turpentine applied to the belly by a flannel "sopped in it, and got a tablespoonful by the mouth." In about two hours began to "roar" with the pain in the abdomen, from the scalding sensations produced by the oil of turpentine, I suppose, which had proved a "severe rubefacient." In some hours she was easy, but the pains returning the following day, she got another tablespoonful, which was occasionally given for three or four days. On the 25th she was put in the same bed with Mary Murray, above named, and on the 26th went home well.

589. The rest of Brennan's pamphlet is filled with accounts of the use of turpentine, in cases foreign to our present subject, and with letters from various persons, among whom should be mentioned John Burns, of Glasgow, and Dr. John C. Douglass. Both these gentlemen speak of his violence of temper, and Douglass concludes his epistle with, "I have only to add that I would feel

more pleasure in making this communication if you did not so frequently indulge your pen in personal abuse."

These letters from Burns and Douglass are answers to Brennan's, which were accompanied with his pamphlet—a publication different, I presume, from the one now before me, but which I have not been so fortunate as to procure. I believe that the Doctor never fulfilled his promise, "shortly to publish a Systematic Essay on Puerperal Fever and the application of Turpentine in the cure of that dangerous malady," &c.

590. I have now presented to you a very fair and full abstract of the essential parts of Dr. Brennan's paper, which I am glad to do, since the publication is rare, at least in this country, and because, as everybody talks of Brennan's method, it is fitting that you, my Students, should have a just notion of what it is.

591. I believe that many of our brethren in this country do still use it in the cases where they cannot see their way clearly, both as an internal dose and as an endermic remedy; but my impression is, that little dependence is placed upon its curative power; certainly no person considers, if even there ever was a person to believe in it, as a revolution in medical practice, effected by the felicitous genius of the quarrelsome Irish physician.

592. Dr. Gooch, *op. cit.* 61, gave it to some out-patients, but had no cause to rejoice in its curative powers, and evidently lost confidence in it. It is true, however, that he used smaller doses than those of Brennan. He informs us that having applied it to the abdomen, in a bad case after venesection, "it had been on about twenty minutes, and I was about to leave the hospital, when my attention was arrested by a clamorous voice from the ward in which the patient lay. On going in with the matron, we found her vociferating most furiously that the turpentine had killed her." But it appears not *quite intirely*, for in a few hours afterward the deep-seated pain and tenderness had disappeared, and the woman recovered her health.

593. Dr. Ferguson, *op. cit.* 224, has no experience of the use of spirits of turpentine; Davis does not praise it; Baudelocque, *op. cit.* 400, remarks concerning Brennan's practice, that of six cases, three died, and two of the others were treated by venesection; and he regards Dr. Atkinson's results as not at all more conclusive

than those of the inventor. As to Dr. Kinneir's success with spts. terebinth., which seems to have been remarkable, Baudelocque is inclined to attribute it to the purgative quality of the drug in the cases. Those successes, he states to have been witnessed by too many Edinburgh physicians to leave any doubt as to the facts. Yet after all, he looks upon it as one of the least "rational" of the methods of treatment, and as chiefly applicable, if to any, to those grave epidemic forms that resist other properly selected means of treatment.

594. That eminent Philadelphia physician, the late Professor Dewees, in his *Treatise on the Diseases of Females*, p. 453, acknowledges his little experimental acquaintance with the use of turpentine in the cases, and is "sorry to say that so far, in our hands, we have not had sufficient reason to rely solely upon it; though as an auxiliary in two or three recent instances we thought it highly useful." He gives, in a foot-note, the case of a Mrs. L., whom he visited in consultation, and considered her hopelessly ill. The turpentine was administered; and he was both surprised and astonished to find, the next day, not only that she was still alive, but much better. The patient recovered. In another case it entirely failed. I called Professor Dewees to assist me with advice in a very bad case, and he proposed the remedy, both internally and externally; but it was in vain that we made use of it. The lady perished.

595. I find that Dr. Arneth, in his late published account of his examinations concerning the state of midwifery in Dublin, makes no allusion to the turpentine practice there. Dr. Blundell, *Princ. and Pract. of Obstet. Med.*, 531, considers that "oil of turpentine does not, in any obvious manner, aggravate the symptoms, and I am not prepared to deny that it may, in some cases, be useful in curing the disease; though it is my decided opinion that, in London, this remedy is by no means so powerful in subduing the fever as the Dublin practitioners have supposed." Dr. Blundell gives four cases, in one only of which was the exhibition of the oil followed by recovery. She had, however, been bled before she took the medicine.

596. Jacquemier omits it wholly in his account of the remedies.

597. You may find yourselves placed in circumstances that shall induce you to resort to Brennan's method. I conceive there will be little objection to your taking such a step, seeing that the inconveniences of the practice are null, and that certain persons do appear to have been rescued, by it, from dangerous extremity. But, as I have taken the pains to open up this whole subject to you, I venture to hope that, whenever you shall please to adopt the turpentine as a remedy, you will do so understandingly, and not, as commonly has happened, from a merely empirical notion, or with a view to the shock.

C. D. M.

LETTER XXV.

SUDORIFICS.

598. I BEG you, before you proceed to read this Letter, to turn to sect. 502, and peruse the account you will there find of Mrs. —, for I wish you to observe that, after I had bled her, I gave her a diaphoretic draught that made her “sweat like a squeezed sponge,” and which I concluded to have been a fortunate circumstance, that contributed materially to her happy escape from danger.

599. Sudorific medicines are among the most reliable of the articles of our *materia medica*, so that, we might say, we are in possession of numerous specifics as to the sudoriferous glands: many of them, too, are extremely simple and innocuous, which is a thing to invite us to the often employment of them. In the terrible constitutional disorders and perversions of function that wait upon the progress of a childbed fever, the skin is not the last of the organs to suffer; nor can man tolerate great departures of it from its normal rate of function without much damage; for the skin is not only the general investment and shield, behind which the rest of the organs are protected in the conflict with the external world, but it is an important sense-organ, for no part of it is wholly destitute of the sense of touch, which shows how vast as well as how indispensable and delicate are its relations to the whole economy. It is, moreover, an exhalant organ, carrying off a large moiety of the watery elements of the body, and along with it great volumes of carbonic acid and salts. The sebaceous follicles of the skin are also charged with the function of eliminating a quantity of the oily residue of the vital operations. See, then, what must be the inconvenience and the mischiefs arising from those modifications of the cutaneous organ that coincide with violent fevers! The suppression of the aqueous exhalation; of the outgoing of the carbonic acid; of the salts, and of the oil, could

not but increase the disorders of any fever, and medicines, therefore, calculated to maintain all those powers in a healthful state, ought to win our confidence.

600. The object usually aimed at in the exhibition of diaphoretic and sudorific medicines is a very simple one—that of procuring or maintaining an abundant discharge of fluid from the pores, and a free evacuation of the materials furnished by the sebaceous glands. Whenever this can be done, the suppleness of the skin is preserved, the heat of the entire body lessened, and a freer course given to the capillary circulation of the surface of the body. It matters little what the agent is, by which these conditions are obtained, except that, as a general rule, the simpler they are, the better they are. Probably, among the diaphoretics, there is none more easily managed, or more sure, than the tartar of antimony and potash, as it is quite tasteless, and may be broken into fractional doses in such a way as almost to insure one against any distressing nausea in using it. Some physicians like to use it in very positive doses, as the fourth, the sixth, or the eighth of a grain. I am not of that way of thinking, and rarely exhibit it in stronger than thirtieth of a grain doses. If you should dissolve one grain of emetic tartar in thirty teaspoonfuls of water, and give a teaspoonful once an hour, I am confident you would not notice many such repetitions without hearing complaints of nausea, and sometimes even of vomiting. I can scarcely ever meet with a sick person taking such doses, who does not complain after swallowing the third spoonful; in which case I usually order ten more teaspoonfuls of water to be added, and try it in fortieths, and occasionally afterwards reduce it to fiftieths, or even seventieths of a grain for each dose.

601. It is, perhaps, desirable, under the usual circumstances of childbed fever, that the woman should be somewhat nauseated, because the state of the nervous system, under nausea, is one to favor a milder or gentler systole of the heart, that seems to me to become, so to speak, always *faintish* or weak, when the sense of nausea is very positive. This condition not only promotes the diaphoresis, but it lessens the force of the vascular injection, and so, powerfully favors any tendency to resolution of the inflamed areas. Antimonials, then, are among our most precious resources; they are truly deobstruent medicines.

602. The golden sulphur of antimony is also an invaluable medicine for the above-named ends, and this kermes, as it is called, has long played an important part in the treatment.

603. With regard to kermes mineral, I perceive that Wood and Bache, in the last edition of the *Dispensatory*, propose the denomination of precip. sulph. of antimony, and say the Edinburgh College name it, incorrectly, the golden sulphuret of antimony. They look upon the precip. sulph. of antimony as "an uncertain medicine, as well from the want of uniformity in its composition, as from its liability to vary in its action, with the state of the stomach." Nevertheless, it has been very largely employed in practice, and still enjoys, perhaps, as much of the confidence of the European part of the profession as any other antimonial—I mean in the cure of inflammation. I myself have long habitually relied upon it in pneumonic inflammations, and have many times resorted to it in the treatment of the lying-in diseases. I have not in my library a copy of Dr. Boer's work, *Abhandlungen und Versuche. Beobachtungen über das Kindbettfieber*, but I have here an excerpt by Dr. Gooch, taken from Boer, who says: "I once lost, in one day, two lying-in women from puerperal fever; the one I attended alone; the other in consultation with the ordinary house physician. At the same time, I had also in the practical school (of Midwifery), two lying-in women ill with the same disease. The next day one of them died, and in the other, death was approaching. It was the seventh or eighth day of her disease; the belly was very prominent, tense, and so painful that she could hardly bear the bedclothes; everything passed away from her unconsciously. She had an excessively quick pulse, with starting of the tendons, and wandered in her talk; her breathing was difficult and broken, and her countenance fallen; the milk in the breasts had gradually disappeared, and almost from the beginning of the disease no lochia were to be seen. With the death of these three lying-in women, to which I already, in anticipation, added the fourth, I was so depressed in spirits, that, in the evening, I sought for consolation in the society of two intimate friends; but I could not divest myself of the fatality of these diseases. Among other thoughts, it occurred to me to try a hitherto unemployed preparation of antimony, which I was assured, in many kinds of fever, had manifested an uncommon efficacy. In

this case, thought I, there is nothing to be lost in the attempt. I went home, provided myself with a dose of it, and reached, about ten o'clock in the evening, the chamber of the dying woman. I found her worse than she was six or seven hours before; but as one of the students and two of the female practitioners were waiting at the bedside, I was obliged to take care not to be seen giving a dying woman a remedy out of my pocket. I gave it, therefore, unseen, to the midwife, who gave it to the patient instead of one of the common camphor-powders, mixed with tea. I then wished the poor creature an eternal good night. When I went the next morning, my first question was, 'When did the patient die?' 'Die!' answered the midwife. 'Early in the morning she begged half a cup of coffee. She sat up in bed, and could not rest till the nurse combed out her hair: I thought her delirious.' When we came to the patient's bed we found her sitting up. She seized me by the hand and thanked me. I knew not, at the moment, whether what I saw was an improvement or only a delusive lighting up from the insensibility of a fatal gangrene; but it was a real amendment." In short, the relation goes on to show that the disease was "gone by magic." In eight or nine days she left the hospital (*vide* Gooch, p. 19). Boer speaks of "twelve or fourteen such desperate cases," cured in the same way; he placed especial confidence in the drug, and "from that time, in our practice, the puerperal fever was never fatal; it both obviated the disease, and cured it if fully formed." Dr. Boer never told what this divine remedy was, and it is asserted, by Gooch, that he was said to refuse to give it to any medical man, with directions how to use it. Was this kermes, that he employed? Was it James's fever powder? No one knows. If it was either the one or the other, no person can believe either of them capable of doing such wonders. If it was something else, which he alone knew, what right had he to refuse to promulgate it, or what reasonable ground to hope for an entrance into the Kingdom of Heaven? A physician who should die with such an undivulged secret, ought to be burned by the hangman, and his ashes scattered to the four winds, as unworthy to rest in the bosom of the earth.

604. Doulcet's method, as you have seen, was to use the kermes after the exhibition of his ipecacuan doses; and I believe the French and German practitioners and continental physicians in

general, are much given to its employment, while in the United States and England it is less used than it should be. If I am to advise you as to this drug, I have to say that, whenever I can feel in the beginning of a case that I have not only done my duty, but satisfied my judgment and wishes as to the plenary operation of my venesection, I shall, for the most part, rest content with the tartar emetic, as above; but, where I have not succeeded in taking away a quantity of blood, to satisfy me that I have made the requisite impression on the inflammatory power, I shall prefer the kermes mineral, and prepare it in the following manner, viz:—

R. Kermes mineral, 12 grains.

Opium, 2 grains.

Nitrate of potash, 30 grains.

Camphor, 30 grains.

Mix, and make a powder to be divided in six doses. One of these powders may be administered every second hour. On many occasions it will be deemed proper to add half a grain or a grain of calomel to each dose. I look upon it as an admirable diaphoretic and resolvent, the sudorific operation of which can scarcely fail, provided the patient should, in the intervals of the dosings, take very liberal draughts, composed of balm, or weak sage, or chamomile tea, or hot lemonade, or orangeade. In case of the supervention of nausea, the doses may be lessened one-half at each taking. Some persons will bear three grains of the kermes in each dose.

605. In many years past, I have hardly failed to cause profuse sweating whenever I have given to sick people the posset drink I described in my sect. 502, which see. I so confidently expect that effect, that I now advise you, as my Students, to resort to it in case the kermes as above should fail to bring out the perspiration. It is a composition not to be despised for its simplicity, as I know full well, after a familiar multiplied experience with it for forty years.

606. Haply, you may prefer to resort to the admirable Dover's powder as a diaphoretic dose. This I conceive to be best calculated as a night medicine, on account of its combining the diaphoretic with its soporific power. Ten grains is a full quantity.

More than this is like to be rejected by vomiting; but if the anodyne and soporific force requires to be increased, you might add a grain of powdered opium to the potion.

607. I invited you, at the beginning of this Letter, to read the case at sect. 502, with a view to show you how I gave a posset-drink, and in what manner it acted in the case; and I now take occasion to say that the physician is always most in the right when he selects the easiest, cheapest, and most commodious means of attaining his therapeutical objects. Pray, then, in some of your future cases, after you shall have taken away the quantity of blood that may seem to be just and right, invite the woman to swallow a tumbler and a half of the plain posset-drink as warm as she can bear it, and wait for six or ten minutes to see if she is not bathed in floods of sweat. If this should be so, then take care to lighten the bed-coverings, lest by excessive artificial warmth you obviate the cooling and calming tendencies of the perspiration, which should be, from time to time, kept up by abundant draughts of hot lemonade, tea, or balm or sage infusion, or chamomile, or even hot *eau sucrée*, which is extremely convenient on all accounts.

608. As to demulcent drinks, as they are called, it is probable that some of them may possess a soothing or calming property depending on the mucilage they contain; yet it is rather to be believed that their supposed demulcent influences are fruits, rather, of the mere dilution; for such liquids, entering speedily into the circulation, must produce a minorative effect through the more aqueous nature of the blood that follows copious imbibition. It has even been proposed and practised to attempt the cure of childbed fevers by enormous imbibition of fluids; and it is surprising to notice what an immense quantity has been drunk daily by sick women. You are reminded by it of Hawthorne's treatment of cholera by opium and toast-water.

609. I should think you would do well, as a general rule, to prevail on the patient to take as large a quantity of liquid as possible; and this means, as large a quantity as can be swallowed without oppressing the *primæ viæ*. In many sick individuals, the power of endosmose seems almost inexhaustible, while in others, it appears to be suspended very early in the sickness. When the drink seems rapidly to disappear from the *primæ viæ*,

leaving no sense of fulness and no gurgling there, the more the patient takes the better; on the other hand, if the absorptive or endosmose faculty is, in a measure, weakened or lost, caution is required on the subject, for any load or oppression of the epigastrium serves to promote the expansion of the local limits and obstruct the resolution tendency of the topical inflammation.

C. D. M.

LETTER XXVI.

BLISTERS—SINAPISMS—STUPES—CATAPLASMS—TURPENTINE, USED AS TOPICAL REMEDIES IN CHILDBED FEVERS.

610. BLISTERS, sinapisms, and stupes, or cataplasms, are resorted to by many practitioners as among the most hopeful means of counteracting the bad tendencies of a childbed fever, and it is an important question how far you should trust to such means of cure. I believe that, by common consent, the use of blisters in pleurisy is admitted to be both prudent and salutary, and I know not if society in general do not place a chief reliance upon them. It is not, however, certain that blisters act as favorably in the serous inflammations of the belly as they do in those of the chest; yet the fact that they do much good in the latter, prompts many of our people to equally confide in them as the former.

611. As you may desire to know what my own opinion on the subject of blisters in our cases is, I must state that I do not think I have ever, of my own motion, used blisters to the abdomen in childbed fevers; and I will go further, and say that I invariably regret their application, having seen many of them used, but few women to recover after them.

612. The object of applying the blister is, I presume, to get at the counter-irritation, on the principle that as *pars dolens trahit*, whatever of cutaneous inflammation may result from the blister, will be so much subtracted from the internal serous area. But, under all circumstances of this sort, there is certainly no man who can do more than guess, or hope, or rather, wish that the serous may wane as the cutaneous inflammation waxes in intensity. The use of blisters here, is one of the clearest examples of the shock-practice, that is, the making a violent or strong impression somewhere, in order to see whether, haply, some new action,

some new disposition, or tendency, may not follow in the economy. It is a sheer *tentamen therapeuticum*.

613. A woman attacked with a childbed fever, is one already overburdened with an intolerable load of irritation, a load which often serves, in eighteen or twenty-four hours, to crush her to the earth. If, by applying a blistering plaster upon the whole abdomen, as I have seen it done, you could have any certainty, or even any strong presumption that you would thereby wholly abolish, or take away the internal causes of the constitutional distress and perversion, I grant you that your blistering would prove a most admirable therapia; but, I am quite convinced that, in twenty cases, the blister will be followed by positive alleviation in not more than two or three of them, and that it will not save, nor help to save the life, in more than one. It is cruel to blister the dying, and cruel to add to those pains which, when we can cure them at all, we can much better control by means not painful and disgusting like these.

614. It has often happened to me to verify, by observation, at the bedside, the truth of Baglivi's assertions as to the mischief from blisters. That elegant Italian scholar, *Opera Omnia*, p. 421, has three chapters, *De usu et Abusu Vesicantium*. In Chap. II. p. 423, he says: "Nihil tam citò, post exhibita supervenit vessicantia, quam sitis cum linguæ ariditate;" and he assures us that this thirst, notwithstanding continual rinsing of the mouth, continues for at least three days, which I believe to be quite a true account. In Section III. p. 424: "Post applicationem vessicantium, observavi nonnunquam motus tendinum veluti subsultorios, et convulsivos, non nullas sudoris expressiones, et quandoque copiosas, pulsus veluti obscurantur, &c., et hæc tamdiu durant, quamdiù in hærent sanguini acres cantharidum sales, quibus mox eliminatis, evanescent," &c. &c. I am confident that few experienced practitioners can have failed to observe the symptoms, so clearly pointed out in the above passages of Baglivi; for my own part, however, I rest convinced that where, in an acute febrile disorder, blisters, being early applied, are seen to be followed by the subsultus, thirst, dry tongue, and copious sweat, instead of finding the symptoms to disappear in about seventy hours, they are far more likely to accompany the case until dissolution alone

puts an end to them. De la Roche, that admirable author, condemns them, and says, p. 208: "That, so far as he knows, no physician has ever thought fit to apply a blister to the belly of a woman in puerperal fever." I would it were so, in fact, but it is not so, and a German writer, Eichelberg, treated his cases with a blister as large as an octavo page, on the hypogastrium, and if that would not cure the poor victim, another of equal size was laid higher up. How many patients could this man rescue from these horrible inflammations by a practice so very detestable? De la Roche himself, on one occasion applied a blister, and had no reason to be pleased with its effects; it appears to me only to have added a new burden to an already insupportable load that destroyed the woman's life. (See his ninth case, p. 293.) He supposes that in cases long drawn out, the use of blisters may, at times, be found useful to oppose certain distant engorgements accidentally arising in the progress of the malady.

615. According to Baudelocque, they are mischievous and inapplicable remedies: he quotes Manningham, at 389, to show that a blister, applied within the first three days, always brings danger, and oftentimes death; also Baglivi's case, which that author relates at p. 423—his third case—in which the blisters did evident mischief, and did not save the woman's life. Jno. Clark is also one of his witnesses to show how they augment the frequency of the pulse. Recamier also is brought up to show that they generally give rise to delirium, with other exasperations.

616. Baudelocque himself adds: "I participate entirely in the opinion of those who reject the application of blisters to the belly or lower extremities in the early periods of the disorder;" but, at a more advanced stage, there may be found motives to employ them. It is quite proper, however, that you should be made aware of the sentiments of Dr. Collins, who, *op. cit.* p. 397, states that "blistering the entire abdomen *after* leeching had been pushed as far as could be, was found serviceable. In some instances the debility, from the very commencement, was so excessive as to induce us to apply the blister at once, using calomel and stimulants at the same time."

617. But I need neither to burden you nor oppress my book with quotations from other authors, for all the authors that have

existed could not make, in this matter, the worse the better reason; and I should think you could desire no stronger argument against the practice than this; that when you have laid on the blister, you have foreclosed and given up or resigned your privilege of making any further diagnostications of the state of parts beneath the blistered area. There is no malady in which it is more essential frequently to diagnosticate the state and progress of the disorder, and your most precious resource to that end is in the palpation of the regions affected. But, with a belly sore as a blister, what fruit can you pluck from such an inquiry? Neither you nor the patient can possibly discriminate between the sensibility of the blistered surface, and that connected with the disorders within. Away, then, with every proposal to blister the abdomen in the first days of a childbed fever, which I regard as both a cruel practice and one discreditable to the taste and humanity of the doctor. While I object, then, to the use of blisters, I can lend no credence to the power of the oil of turpentine poured over flannel laid on the sick woman's abdomen, according to the method recommended by Brenan, already detailed in Letter XXIV.

618. Fomentations, or stupes, as they are called, and cataplasms, which give no pain, may be very advantageously prescribed. Almost invariably the patient acknowledges a certain degree of relief and comfort from them, and willingly suffers them to be from time to time renewed, provided they be not too heavy, and not so cool as to chill the skin.

619. I should think no better poultice can be made than one of pure warm water imbibed in a large, soft, and well-doubled napkin or square of flannel; to be spread over the sore region, and then covered up with a square of oiled silk; which should be used both to restrain evaporation, and to prevent the moisture from spreading into the dress, or the bed-coverings. Those who, from prejudice or other motives, prefer poultices, employ the light mixture of Indian meal boiled in water to the consistency of mush. A teacupful of hot mush laid betwixt folds of flannel, and spread out thin, may be sprinkled with a tablespoonful of laudanum, and then laid on the part; or, hops soaked in boiling vinegar and water and laid in a flannel bag might, by some, be preferred. Others will use the powder of lintseed, &c. &c. It matters little what the stupe

or cataplasm be made of, provided, always, it be only moist and warm enough to fulfil the indication, which is to maintain the humidity and suppleness of the parts lying above the diseased areas. I cannot venture to advise you to employ the treatment recommended by Brandis, of Hanover, in 1798, which was to cover the belly with ice. Yet, what more probable topical remedy could you apply than this? I say, I cannot advise you to adopt it, since, I believe, a single case so treated in this country, and resulting fatally, would suffice to ruin the professional reputation of whosoever might venture to put it in practice. Physicians, unhappily, like other men, are too often compelled to bend to the power of popular opinion, or rather of ignorant prejudice. I regard the universal distrust of cool applications in our cases, as one of the existing remaining influences of the doctrine of milk-metastasis. You should be aware of the excessive cautions, advised by Busch, as to the warmth of the sick chamber, the avoiding of draughts of cool air, &c., for even the wisest physicians are bound by the chains imposed, I verily believe, rather by public sentiment than their own unbiassed feeling and judgment.

620. In my next Letter I shall lay before you certain opinions that have been, in different quarters, maintained as to the tonic medicines, and seize the occasion to speak of cases which, though generally ranged under the class of childbed fevers, ought, unless I am in error in such an opinion, to be collocated with other classes of diseased action; and I rest now, with the greatest regard, &c. &c.

C. D. M.

LETTER XXVII.

621. "THE extreme difference of opinion, and the very opposite measures recommended by practitioners, arise chiefly, I am satisfied, from their treating of every variety of puerperal fever as one and the same disease; whereas there is not, perhaps, any other which exhibits a greater diversity of character in different situations, and even in the same situations at different periods. In some, the fever is accompanied by symptoms indicative of the most active inflammation, such as to forbid the least delay in the free use of venesection, and the decided employment of antiphlogistic medicines. This form of disease, *which is by far the most manageable*, is generally met with in private practice. Puerperal fever, when epidemic in hospitals, is directly the reverse; at least, in four epidemics which I have witnessed, the symptoms were of the lowest typhoid (typhous?) description, the pulse being so feeble as to make you dread, in many, even the application of leeches; the patients, in several instances of this form of the disease, exhibiting somewhat the appearance of those laboring under cholera."

622. I have here, my young friends, presented to you an excerpt from Dr. Collins's *Midwifery*, which you may find at his 390th page, and I now ask you to consider, whether or no these diverse forms of childbed fevers can possibly be one and the same disease, arising from the same causes, and governed by the same principles of nervous impression and reflexion. I ask you to consider whether there is not a probability, so great that it passes into certainty, that the principles of these diverse maladies must be totally different from each other, and whether Dr. Collins has not described as childbed fever cases, in his hospital, affections having no claim to that title, but rather to those classed with true zymotic typhus.

623. Unless I am a man grossly deceived and misinformed by my studies both of books and circumstances, it will be impossible to make, out of these *foudroyant* forms noticed by Dr. Collins, as well as by numerous other writers, anything but malignant typhus, with which, as an accident there has come to complicate the zymotic disorder a topical inflammation usually appertaining to the whole group of childbed fever phlegmasias, and which, once established upon this typhous basis, run only the more rapidly their career of devastation as the effusion, suppuration, or gangrene of childbed fever.

624. All the writers, I think, insist that these typhous forms are to be met with only in hospital practice, where, as you observe by the excerpt from Dr. Collins, it reigns coincidently as to times or seasons, with the inflammatory forms as they are called; by which, I conceive, you ought to understand is meant a childbed fever inflammation and not a true typhus fever carrying along with it a *putrescentia uteri* or a galloping inflammation of the peritoneum. The instances of this raging typhus complicated with lying-in inflammations, are very numerous in our records, and while I should not find it expedient to present here an endless citation of them, yet I shall beg to say, that Dr. John C. Douglas, of Dublin, who spent several years in the lying-in hospital there, and was familiar with its incidents and transactions during many years, gave us his *Report on Puerperal Fever*, in answer to queries addressed to him. It is in *Dublin Hospital Reports*, vol. iii., at page 145: *Query 4*. "Have you observed any connection between puerperal and common epidemic fever?" *Answer*. "Although I am satisfied that puerperal fever, in all its forms, may be generated in a lying-in hospital by local causes, I have no doubt of its being often excited by atmospherical influence, like common epidemic fever; and I am of opinion that the same exciting influence which would, at another period, produce common fever in an individual, might, at the time of lying-in, produce puerperal fever." This seems to me to be a very clear admission that the zymotic cause or causes of typhus, ought to be regarded as the real cause of those fatal attacks by which lying-in institutions are scourged to that degree that they are rather fitted to be qualified as slaughter-houses for poor lying-in women, than as the beneficent asylums

of the distressed. Should you concede that those mortal forms of the lying-in disorder called hospital childbed fever, are really based upon and governed by the pathological principles of true malignant typhus, then I think you ought to banish them from our category, and class them rather among the zymotic diseases, and that no woman perishing under those forms should be numbered among the victims of childbed fever, but rather cast among the hecatombs of typhus victims. If your patients, lying in during epidemic smallpox and yellow fever, should perish with either of those disorders, but with childbed fever complications, would you not report them as victims of smallpox or yellow fever? Wherefore, then, during the reign of equally malignant typhus, should you report your cases as childbed fever, and not as cases of epidemic typhus?

625. In the remarks I offered to you in a former Letter, I showed you that women sometimes are observed to hasten through all the phases of our disease with a surprisingly rapid pace, exhibiting after death appearances of a complete ramollescence of the womb, or putrescence of that viscus. We may well comprehend that, in certain circumstances of the general health, an inflammation of the more delicate bloodvessels and capillaries of the womb should be capable of suppurating as rapidly as an ordinary gum-boil, and so, of reducing the whole viscus to a state of softening, amounting to disintegration of the entire textures of it. That a woman just delivered, and subjected to such frightfully rapid inflammatory dissolution of the but now vacated womb, should succumb, and with symptoms closely resembling those of malignant typhus, ought not to give rise to any wonderment; and it is perfectly conceivable that she should thus die with a pure and simple phlebitis of the organ, without suspicion of typhus complicity.

626. That a death so rapid as that just mentioned, though it might exhibit phenomena precisely similar to those attendant on typhus fevers, with uterine, or peritonitic, or phlebitic accidents, depends upon a wholly different principle in pathology, ought not to be questioned. Typhus fever is not a disease of the gastro-enteric mucous membrane, nor of the liver, or the spleen, or reproductive organs, or kidneys! Typhus is an affection de-

pendent upon modifications primary in the nervous centres, or in the nervous mass; one in which the powers of the nervous tissue, a form of albumen, are so changed, or reduced so low, that the organs and tissues over which it presides, and whose functions, and indeed very life, are absolutely dependent upon its creating, conserving, and governing forces, fall into a confusion and disorder proportioned to the greater or less abandonment in which they are left when thus bereft of the centro-peripheric force. Typhus rules at the nerve-centres. Inflammations rule at the opposite poles, among the organs and textures, of whatever name. The brain, says Oken, is the sun—the centre of a system; the organs are planets revolving around this common centre, and drawing thence their life and activity through nerves, that, like illuminant and calorific rays, connect them with the sources of their life and power.

627. Typhus is centro-peripheral; phlegmasias are periphero-central maladies; and the difference between them is as great as that betwixt the positive and the negative poles of a galvanic battery.

628. There are in our obstetric literature many records of epidemic childbed fevers, so called, in which *all* the patients perished. I have already shown you that of thirty-two, Dr. Wm. Hunter, a learned, dexterous, and bold practitioner, lost thirty-one women with the disease in his hospital; and in an epidemic in Normandy, in 1767, D. Lepeey is reported by Eisenmann, p. 150, to state that all the subjects perished. This same Dr. Eisenmann, of Munich, has given us a work, *Die Kindbettfieber*, of the greatest learning and research, one which no man can peruse without admiration of his science, however one may hesitate to admit his therapy. He devotes many pages of his second part, commencing at p. 149, to the discussion of what he denominates *Typhus Puerperarum*, or *Puerpero-typhus*. Dr. Eisenmann does not, however, conceive of this puerpero-typhus as original typhus, but looks upon the cerebro-spinal symptoms as accidents or secondaries, rather than as chiefs or primaries in the diseased processes. In maintaining this opinion, Dr. Eisenmann must be classed among the believers in childbed fever, a notion against which I have too often protested in these Letters to make it proper for me to repeat any arguments

in this one. The fact that epidemics have existed in which all the patients perished, is a fact to prove that they could not have died with inflammations, pure and simple; for inflammations can never, in any epidemic, prevail with such invariably mortal power. There are too many differences and varieties in human constitutions to make such a universality of deaths possible in pure phlegmasias.

629. On the other hand, I can discover no reason why the epidemic cause of puerperal typhus, or puerpero-typhus, as Eisenmann hath it, should not cut down both strong and weak alike; for the causes of typhus, whether they be natural or substantive contagions, are poisons for all manner of persons; and those poisons, when intense, either from condensation or from some natural quality or power they possess, may be conceived of as capable of destroying whatever persons they encounter. But this is surely not possible as to inflammations.

630. Let me advise you to procure, at the first opportunity, a copy of Schleiden's delicious book, called *The Plant*; a Biography, printed at London in 1853, and to read in it the eighth Lecture, and in reference to the subject now before us, particularly the 206th page relative to the Javanese "Valley of Death." After describing the intricate forests, the frightful steeps and volcanoes, and strange poisonous vegetable productions of the place, he proceeds: "Large tracts are desolated by the action of the great volcanic phenomena. But new life springs up everywhere, side by side with destruction, and once more clothes the naked earth. Only particular regions make exceptions to this rule. Leaving the thickets of the old forest, and climbing a moderate hill, suddenly, in a narrow flat valley, a horrible wilderness, a true palace of death spreads itself out before the eyes of the shuddering wanderer. No trace of thriving vegetation screens the sun-scorched earth. Skeletons of all kinds of animals bestrew the ground. There is it often seen how the terrible tiger, in the moment when he has seized his prey, is himself overtaken by destruction; how the bird of prey, hurrying to feed upon the fresh carcass, falls into the maw of death. Dead beetles, ants, and other insects, lie in heaps around, and testify still more, how apt is the name, 'Valley of Death,' or 'Poison Valley,' as these places are called

by the natives. The formidable character of these localities arises from the exhalations from the soil, consisting of carbonic acid gas, which, on account of its weight, is a long time diffusing itself in the air; exactly as in the celebrated *Grotto del Cane* at Naples, or the vapor caves at Pyrmont. This gas causes inevitable death by asphyxia, to all near the surface of the soil. Man alone, to whom God has given it to walk erect, traverses usually uninjured these deserted tracts, since the poisonous exhalations do not reach up to his head." Can not you, young gentlemen, conceive of a substantive though impalpable, invisible cause of typhus, spreading itself abroad as an exhalation within the wards of a Hospital, and acting as mortally and undistinguishingly as M. Schleiden's vapors of the Poison Valley?

631. I expect here to be met by the question, What difference does it make, whether your childbed-fever inflammation, or your puerperal typhus, be the disease that cuts off so many women? Are they not all childbed cases, or lying-in diseases? Such a question, though nothing more nor less than a cavil, should be answered according to the imagination or convictions of a man. In an epidemic typhus fever, many lying-in women may perish; and though they give manifest signs of the existence of the phlegmasias I have so much spoken of in these Letters, they have no claim, on that score, to be ranked among the childbed-fever cases. They are only lying-in diseases engrafted upon a horrible basis of typhus fever, which is an affection, primary in the cerebro-spinal axis, or, if one would but speak the whole truth, in the nervous mass, wherever it exists within the boundaries of an animal economy. It would be quite as true to say, that in an epidemic of Syrian plague, all the pregnant and lying-in women dying of the disorder, do perish with childbed fever; and it is probable, indeed, that they do so—but the phlegmasias are the secondaries—the accidents falling out in the course of the plague cases. The same is true of epidemic smallpox, and a multitude of other disorders. But such accidents, such intercurrents, are far from nullifying the truth, that those women are victims to smallpox, or plague, &c., and not to childbed fever.

632. Kiwisch, at p. 564, *op. cit.*, protests against the gathering together into the group of disorders called puerperal fever all the

febrile affections that may chance to lying-in women, and considers that they who would do so will render no service to the cause of science.

633. Dr. Schleiden, in this relation of the Poison Valley, seems to me to foreshadow a future explanation, or rationale of epidemics, which, in all the real, great, wide-spread instances of them, appear to me to depend upon exhalations, not merely from the soil, but from the far-under depths of the earth, below the lower strata, where the boiling, incandescent materials of the globe evolve principles, poisonous as those of the Valley of Death, and destroying us, sometimes, by a force analogous to asphyxiation, and at others by volatilized mineral substances and gases, of whose real nature we have, as yet, no knowledge. A man must be very credulous to believe, because the reagent and the eudiometer can detect no foreign substances in the atmosphere, that none such can exist there, and that chemists have already attained to a final term or limit, in their daily lengthening catalogue of simple elementary substances.

634. Is typhus a contagious disorder? To this question I am not prepared with knowledge that should warrant me to assume the privilege of a judge, and to decide either affirmatively or negatively. As far as my own private opinion goes, I am free to say that I much doubt of the contagiousness of typhus. But as my opinion is that of only one man out of the thousands of our Class, who have as good a right as I to judge, it is a matter of the greatest indifference what bias I may have taken on the question. If I could *know* that it is, or is not contagious, it would be another thing. I do not know; I only believe that it is not contagious, in the sense of a virus, effluvium, exhalation, or substance, generated within or upon the bodies of the sick, and capable of producing a similar specific disorder in other persons.

635. As to the contagiousness of childbed fever I have not only expressed my deep inward conviction, but have furnished you with the arguments and facts that have resulted in that conviction as to myself. I shall not, therefore, say anything further on the subject. Can those arguments convince you? Time alone can show what it is that you shall admit or deny.

636. But at last, and always, the question must come up con-

cerning the treatment to be pursued in these instances of complication—complication of typhus fever with the various forms of inflammation assumed in the group called childbed fevers.

637. It is melancholy to think that our art has so little power to intervene beneficially in these unhappy instances; and I am of the same mind with De la Roche, who looks upon the lancet as the chief resource of the physician, and, in a foot-note at p. 159, asks, “Mais si les malades respirent dans une atmosphère souillée d’exhalaisons putrides, sans qu’à cet égard on puisse rien changer à leur position, comme cela se voit dans certains hôpitaux, il est très possible que la méthode de curation que j’expose ne soit point celle à laquelle on doit alors donner la préférence; et surement il ne conviendrait pas d’employer la saignée aussi hardiment que je le fais. Mais quel est le traitement à suivre dans de telles circonstances? *Je n’en sais rien.*”

638. This answer of De la Roche ought to be in the mouth of every man of us, to whom the question is addressed: *Je n’en sais rien.* The hospital returns of deaths in lying-in women during the prevalence of epidemic childbed fevers in them, all show that, as to the cure, the answer of every great celebrity in Physic is, “*Je n’en sais rien.*”

639. I have had hospital cases of childbed fever under my care, and I bled them against all protests, and even with fear and trembling sometimes; nor did my professional conduct on those occasions leave in my mind any sting of remorse. I have had many occasions to treat typhous forms of childbed fever in the city. I bled the patients, and, though I have not cured all, I remain, in a somewhat advanced stage of life and clinical experience, profoundly impressed with the thought that it was the best remedy in my power to offer; and the whole result of my ministry in this way, gives me to-day no heartache when I examine myself to learn what it is I have done.

640. Will you cure these typhous forms with opium? You cannot.

Will they yield to the power of ipecacuanha? No.

Will you sustain them with cinchona and wine, meat-broths and jellies? Try them.

Will you treat them with camphor, with oil of turpentine, and with blisters? What nonsense!

Will you venture to attack the local malady with leeches and cupping-glasses? But, to do this is only to bleed! You only will cheat yourself, and, perhaps, do some good to others.

641. Is it true that all typhus is, invariably, aggravated by venesection? Why should it be so!

What is the result of the treatment of the typhus fever, complicated with childbed-fever inflammation, when conducted on the principle of stimulation? Is it not death?

How else will you cure it? *Je n'en sais rien.*

If you will not cure them by the lancet, what will you do? *Je n'en sais rien.*

642. Suppose your case of typhus, with childbed fever engrafted on it, breaks out in hospital, or in the country, will the patient die for the abstraction by venesection of one ounce of blood? No; two ounces? four ounces? six? eight? ten, perhaps? De la Roche says, p. 163, "In doubtful cases I think one never risks much in drawing away a very small quantity of blood by way of experiment. If, after this evacuation, the pulse seems to be better developed, and the blood shows a degree of siziness, one should not hesitate to take more. If, on the contrary, the pulsations are enfeebled by it, and grow unequal and irregular, a new bleeding would be, at most, useless; and, do whatsoever we would, it is probable the patient's life could not be saved." De la Roche was a very sensible, wise man.

643. I am not one of those to believe, because Moses, the law-giver of ancient Israel, has said that the "blood thereof is the life thereof," we must always do mischief by taking *some* of it away. It does not make any difference, as to the life, whether a person shall have twenty pounds of blood in the vessels, or only nineteen pounds and a half; and if I can suppose that by taking away half a pound out of the twenty, I shall cause the pulses to beat in a way more favorable to the progress of the disease, and even hopefully as to its cure, I shall certainly venture to abstract it. Therefore, even when I am standing in presence of typhus, complicated with one of those horrible childbed-fever inflammations, I shall overlook the typhus, and attack the local disease

by the only valid measures that are left to me under such circumstances. I am not here speaking to you as if I should do this with confident hope or belief in the power of the measure to cure; but I should do so, only because, as to all other hope, there is not one, except perhaps that sort of chance-medley, wherein people do recover, like Boer's woman, in section 603.

644. Such patients require nourishment. But it is a common error, I believe, in our ministry, to inquire how much of this or that food the patient has taken, and to infer that there is so much nourishment, because so much deglutition. There is a wide difference between swallowing the food and drinks, and profitably employing them afterwards. We should reflect that in these dreadful inflammations, with fever, the blood is rapidly losing its healthy crasis, and that a few days of such fever may suffice wholly to ruin it, for the hæmotosis is not only a power to develop blood, but to maintain that developed stage of it. In these fevers, the hæmotosis is as much deranged as the digestion, the biliary function, the renal, and all the rest. Therefore, let us not count upon the ounces of soup, or the pounds of barley-water swallowed, as so much gained in the case. The very endosmose itself appears to me, in bad cases, to be suspended, which is the reason why we so often see the accumulated ingestions of many hours suddenly discharged from the stomach by vomiting, or else running off in the form of colliquative diarrhœa; whereas, the discharge is, mainly, the unabsorbed portions of aliment or drinks. I am very desirous that my Students should, in feeding very ill people, make such reflections as those above, and not add to the mischief and hopelessness, the still more abominable burdens of beef-tea, wine-wheys, rice and barley-water, &c., that are so wrongfully forced upon the reluctant and revolting organs of the sick. True it is, the patients do require to be sustained, but they cannot digest beefsteaks, nor Liebig's maceration of beef in cold water. Certainly, if Liebig's broth, or chicken soup, or oyster liquor, and such like things, must be given, they ought to be given in very small quantities indeed, while pure water, or water acidulated with lemons or oranges should be allowed *ad libitum*, with the sole oft-repeated protest—Don't drink too much, lest you add to the distress and danger.

645. A woman lying ill with a childbed fever, should be instructed that it is dangerous to abandon the recumbent position, one in which she should be carefully kept until the disease is absolutely judged. A case might be, by one of you, happily conducted to a stage in which resolution should be on the point of taking place; that is, it might be so situated as to make it almost certain that this fortunate resolution should occur, and yet, all these benefits be dashed aside by imprudently rising up or leaving the bed, even for a moment. A very temporary rest in a sitting posture might so change the sanguine determinations within the belly as to insure a subsequent and speedy effusion. I advise you, therefore, invariably to direct that all the evacuations, whether by stools or urine, should be with the use of the bed-pan. To rise and sit for a few minutes by the bedside would be to expose the surface, and more particularly the extremities to difference of temperature that might have a fatal influence.

646. Any lying-in woman ought to be protected from shocks and strong emotions of all kinds. The countenance of the physician, and the manners and words of the attendants should be studiously of a kind to inspire confidence and hope; and, in order to that end it is not necessary to be false to the woman, for no one knows at the commencement what shall be the issue of the illness. We may, therefore, feel authorized, however great are our doubts and misgivings as to the success of our ministry, to set in plain array all the motives to hope and even expect a happy recovery. Such conduct may haply give important aid to a general treatment; whereas, a gloomy and anxious, timid look on the part of the medical attendant, and his assistants, would but add depressing influences to the exhausting perceptions that are striving to sink her into the grave.

647. As to the ventilation of the apartment, I should think there can be almost no disease in which a free circulation of air in the sick chamber is more important—seeing that the respiration soon becomes greatly troubled and embarrassed by the distending tympanitis; for, as I more fully explained in sect. 275, it must always happen, when the tympanitis is very considerable, that the woman shall either employ a smaller quantity of oxygen than is compatible with a perfect aeration of the blood, or the

breathing organs must exercise their power at a greatly increased rate. Hence, I say an impure confined atmosphere must be to the last degree unfriendly to the woman. It is, surely, a subject quite worthy to engage a share of your attention, that of regulating the chamber, as to the position of the sick woman's couch, in such a way as to avoid draughts of air which would be unfriendly in such disorders.

C. D. M.

LETTER XXVIII.

FEBRIS GRAVIDARUM ET PARTURIENTIUM.

648. SOME of our writers have spoken of a febris gravidarum et parturientium as an affection not very unfrequently to be met with in practice; and my own clinical experience has furnished me with various occasions to observe its appearances.

Inasmuch as women, when recently confined, are extremely liable to be attacked with different forms of the group of disorders called childbed fevers, we might, *à priori*, suppose that the foundations of those affections are, many times, laid before the birth of the child. There is, therefore, a feverish condition which is met with in many pregnant women advanced to near term time, and which, previously to the attack of labor, is mistook for intermittent, or interpreted as an hectic irritation dependent upon the pregnancy, yet which, soon after confinement, bursts out fearfully as childbed-fever inflammation, in some one of the forms of that group of phlegmasias.

649. Dr. Meissner, *op. cit.* 218, treats of the fever and inflammations that attack pregnant women, and justly remarks upon the complicating power of pregnancy over the rates and tendencies of such affections; and indeed I consider that the Student should look upon all the maladive conditions of a pregnant woman as cases of complex disorder; since the presence of the ovum in utero, the instant necessity for continuous acts of uterine evolution, the consumption of material in the uterine and foetal development, are all circumstances that tend to be interrupted by, or that tend to interrupt, bias, and modify any general feverish state, as well as any topical inflammation. For the pregnant woman, the first necessity is to complete the evolution-acts; and in order to that end the health ought not to be disturbed in any way, since whatever disturbs the equable operation of the constitutional

powers acts directly upon that most delicate of the life-functions, gestation. Either to lessen the progress or to augment the rate of the uterine evolution beyond the normal ratio is, alone, sufficient to endanger, not the process only, but the life of the woman herself. Fevers and inflammations could not but have such a tendency. The observations on these topics, that you may study with greatest profit, are those of Prof. Jörg, in *Handbuch der Krankheiten des Weibes*, cap. 25, § 569, u. s. w.

650. It is clearly asserted that a woman may be seized with putrescentia uteri so as to die within eight hours after the completion of the labor, or the abortion; for it occurs even in very early stages of pregnancy; nor have we reason to doubt that, as putrescence of the womb is found to affect, in some cases, only small portions of the organ, the existence of the disorder during pregnancy might lead to a hasty evacuation of the womb; nor that the unaffected muscular portions should be able to exert so much expulsive power as to complete the delivery, soon to be followed by fatal aggravation of the lesions precedently established.

651. I consider you would scarcely err, should you assume it to be probable that all those rapid dissolutions that I have mentioned in these Letters, sections 225, 230, 251, 273, and of which you may find abundant records in the medical library, are really cases in which the inflammation had gone far before the labor began, and that exploded, so to speak, as soon as it was transacted, being precipitated and driven to a mortal term by the violence of excitement through which the womb had just passed.

652. In the course of my own clinical experience, I have observed several examples of slow febrile disorders that had lasted many days before the labor came on, and in which, after the birth of the child, I had to contend against what I regarded as an inflammatory fever. In certain of the cases this inflammatory fever developed itself as fatal childbed-fever inflammation, under the form of uterine phlebitis with peritonitis. The signs, during the progress of the attacks, were not to be misconstrued; since not only was the pain, the ballooning of the belly, &c., to be observed, but the pyæmic intoxication, and the death, at last, with black vomit. I have already, in a former Letter, alluded to the unfortunate case of a lady here, who perished, as I believe, from

too rapid an evolution of the womb by dropsy of the ovum, that gave rise to a sort of eruptive or bursting development of the ovum.

This day, September 18, 1854, visited a young lady who was delivered of her first child at 3 o'clock A. M. of the 17th inst. She was at term. On the 16th, she sent me a message, late in the evening, to say she was in labor and would call me in the night. I was not called, however, until the next morning at about 10 o'clock, when I found her affected, every three or four minutes, with the most violent pains and bearing down, as if in advanced labor. She suffered very severely. I observed that the uterine globe did not become very hard during these pains, and, upon touching, discovered that she was not in labor, but affected with *rhumatismus uteri*, which, it is true, might at length bring on the contractions, *ad partum*. The pulse was very frequent—at 145. I gave her directions to be bled at the arm, and left her. At about 9 o'clock P. M. I was hastily recalled, being told she was now much worse. I again touched, and found the os and cervix wholly unaffected. Ordering the nurse to give an opiate enema of forty-five drops of laudanum with starch-water, to be repeated in an hour if unrelieved, I again left, and was recalled at 1½ A. M., when I found the child's head quite out of the circle of the os, and pressing the perineum outwards. It was born at 3 o'clock, and all was well except the pulse, which was 140. She was a good deal fatigued, and, as she had been bled shortly before, I did not open the vessel again. All day yesterday the pulse was flying; the skin not very warm; the intelligence and temper perfectly natural; and she had no hypogastric pain on pressure or movement. I gave her a small portion of oil, which procured two dejections, and to-day the pulse is normal, and my patient in every respect as well as I could wish. What was the matter with this young woman? What caused the pulse to fly at the rate of 145 beats to the minute? She was not frightened, but, on the contrary, courageous. She had *rhumatismus uteri*, no doubt, before the child was expelled, but not to a degree sufficient to account for so extraordinary a circulation. There was nothing extraordinary as to the brain, lungs, or alimentary organs. What was it? I feared at the time it would prove to be *febris gravidarum et parturientium*, to end, probably, in fatal phlebitis. Was there not

in the young lady a state of the *membrana vasorum communis*, of greatly exalted life, that should account for those strange phenomena?

653. A *febris gravidarum* ought, in my opinion, to be invariably looked on as an endangial inflammation. It is a real disorder of the bloodvessels, and its seat is chiefly in the sanguiferous vessels of the reproductive system of tissues. It is this that renders it dangerous; a danger not a little enhanced by the uncertainty and even impossibility of making early diagnoses. The diagnosis is an *ex post facto* determination in these maladies. As inflammation may attain to, and for a long series of days, maintain any certain stage, it is reasonable to think that, in these vascular phlegmasias, affecting the most delicate vessels, fever should arise and continue with little augmentation for many consecutive days during the last processes of a gestation; and that the incidents and the consequences of the labor should, in certain instances, allow of their ready and perfect resolution, or in others, hasten them on to a fatal termination.

654. The example we have in crural phlebitis, vulgarly known as milk leg, is a case in point, to prove to you that a very great and dangerous inflammation of important veins may arise and come to a conclusion during pregnancy, which is not interrupted by it. There are here two ladies, each of whom suffered from a most violent crural phlebitis, that came on in each of them six weeks previous to the accouchement. In both of them, the inflammation was cured and the children afterwards safely born, both the mothers recovering without the least lying-in accident. I saw a lady here with a saphenitis, that came on a few days before the birth of her child. I regarded her position as most perilous when I first inspected the limb—for unhappily, the malady had already reached the pyæmic stage when I took charge of the case. She died a few days after delivery, with all the usual accompaniments of purulent infection of the blood. This was the second fatal case of phlebitis of the saphena system that I have met with. In the first, the pyæmic intoxication sometimes amounted to mania of the greatest violence.

655. In treating the febrile diseases of the closing days of pregnancy, I have, then, to advise you to keep your eyes open to

the signs that indicate phlebitic disturbance ; and I cannot give you any more friendly counsel than this, viz : to ask you to reflect upon my propositions as to the possibility and the existence of that state which I have so repeatedly pointed out under the denomination of endangitis, or inflammatory excitement of the true bloodvessel ; *i. e.* the inner bloodvessel—the *membrane commune* of Bichat—*membrana vasorum communis* of the authors.

656. Keep this thought before you in diagnosing the late fevers of pregnant women ; and instead of drugging your sick ladies with calomel, you will command them to lie in bed, to bathe in warm water, to diet rigorously, to use camphor and kermes, and nitrate of potash, or tartar emetic solution, or opium in some of its numerous formulæ ; and more than all that, you will attack the enemy in his castle, and expel him at the point of the lancet. Never be so foolish a doctor as to be afraid to bleed your patient, when you think it safe and convenient and in her interest, to do so, because you may be surrounded by chattering women or doctors who are already half ruined by Homœopathic propensities. Keep the bowels in a soluble state ; visit the patient often ; enjoin strict, protracted recumbency, and bleed her so that she cannot inflame. Such is the method I have to commend to your consideration ; such is that I should pursue this hour if the case were now under my care.

C. D. M.

LETTER XXIX.

CRURAL PHLEBITIS, COMMONLY CALLED MILK-LEG.

657. THERE is a disease for the full comprehension of which we are indebted to that admirable gentleman, Dr. Robert Lee, of London, who has done so much to promote medical progress in obstetricy for these many years past; a man always on the side of reason, truth, and moderation. You should all possess you of his *Lectures* and his *Treatise of the Diseases of Women*, and his little excellent duodecimo of *Clinical Midwifery*, &c. &c., books that are replete with useful clinical instruction. In those works you will find an excellent literary, and critical, and clinical, and therapeutical history of the disorder called milk-leg, but which, thanks to his worthy industry, is now clearly known by the title of crural phlebitis; for he it was who showed to the present age what physicians never learned before, that phlegmasia alba, or phlegmasia alba dolens puerperarum, or milk-leg, is not a disease of the areolar tissue, or of the lymphatics of the leg, but a cruel and very dangerous inflammation of the veins, sometimes the great crural, sometimes the superficial or saphena system, and, in certain cases, of both together, extending high upwards in the iliacs and cava. I should not undertake to speak of it here, were it not that I cannot but regard crural phlebitis as one of the forms of the puerperal phlegmasia that constitute the burthen of these Letters. It is true that a woman may suffer from crural phlebitis without hazarding the integrity of the uterine veins; yet it not rarely happens that whether the original areas of the phlogosis are laid in the membrana vasorum below Poupart's ligament, or even in the saphena, or whether they commence in the external iliac or even the vena cava, the phlegmasia spreads creepingly into the vessels of the new-delivered womb itself, whereupon we soon detect the existence of a metro-phlebitis.

658. My own clinical observation of crural phlebitis induces me to rely confidently on the curative power of proper treatment early begun; but that cannot be, except for a medical attendant who is as vigilant and inquiring as he ought to be.

659. When a crural vein, or an external, an internal or primitive iliac, or a saphena, begins to inflame, no pain is felt, nor is there any rigor or any speedy reaction of fever observed. I say, when the endangium begins to inflame. Not so, however, when the inflammation has begun to affect the basement tissue which sustains the true vessel. Here, there is tension, pain, obstruction of the efferent function of the vessel, swelling, heat, and rigors, with fever.

660. This I conceive to be a correct statement of the case, and this it is that, seeing the early diagnosis is difficult if not impossible, leads to so many painful results; for it is to be believed that an educated physician, could he know of the founding of the incipient areas of inflammation, could and would cure them at a blow.

661. Let me here repeat that vascular phlegmasias are the bane of pregnant women, and that they constitute a large percentum of the serious maladies to which they are subject. Hence, it seems to follow that whenever you find a pregnant woman complaining of febrile symptoms, pains, and inabilities, which you cannot at once diagnosticate, and so, *locate*, if I may use the phrascology, I think you ought to suspect some phlebitic malady, and on that hint direct your investigations.

662. In my own case, whenever I am in this way embarrassed, I can in general readily resolve all my doubts by pressing the soleus and gastrocnemius muscles suddenly and strongly against the bones of the leg. If the crural vein, either below Poupart's ligament, or the external iliac above it, are in a somewhat advanced stage of phlebitis, the pressure of the calf of the leg, as above explained, invariably draws forth a sudden cry or complaint, though the woman never before suspected any ailment of the limb. This is the first step in such a diagnosis. The next is to ask permission to explore the state of the crural vein, in doing which, the woman should lie on the back, so as to enable you to trace the course of the fasciculus of vessels coming out from the belly. In a

somewhat advanced stage, this tract is found to be sensitive and somewhat enlarged, as if a cord were running in a spiral direction from the semicircular edge of the fascia lata, inwards and downwards, along the thigh. The detection of such a swelling, coinciding with tenderness of the calf of the leg, settles the question; it is a crural phlebitis; and if you allow it to proceed to the extent of what is called pyogenic inflammation, the woman, in all human probability, will die with the disease.

663. Sometimes the phlebitis has not descended yet to the thigh. You can detect the swollen and painful cord, where it runs along the margin of the pelvis, by pressing the iliac region with the hand, after directing the thigh to be somewhat flexed.

664. In my opinion, these cases will be detected at a period when it is too late to bleed as a method of cure; nevertheless, provided you should judge it not already too late, and the state of the constitution should warrant the act, I hope you will freely bleed every and any such patient that may come into your hands. But where you cannot resolve to do so, you ought to apply a row of leeches along the course of the swollen vessel, not in the vain hope by this alone, to cure the phlebitis, but as a means of moderating the heat, pain, and tension of the areolar sheath, in which the inflamed vein runs.

665. As soon as the leech-bites have become dry, a narrow strip of blistering plaster, say an inch and a half wide, by three and a half to five and a half inches long, should be laid over the track of the vein, and maintained by an adhesive strap until the blister rises; to be dressed with basilicon, kept *in situ* by an adhesive strip.

666. The woman should be commanded not to quit the horizontal posture until the disease is fully judged, nor, indeed, until the resolution of the inflammation is completed.

667. The leg should be laid upon a long pillow or bolster, so adjusted as to give the limb an inclination upwards of some 8° , or at most 10° , and it should be kept so day and night.

668. An old petticoat of flannel should have its hem cut off, and the gathers also, in order to make a stupe-cloth. This *old* flannel being wrung as hard as possible out of a *hot* mixture of vinegar and water, equal parts, should envelop the limb from the toes to a

point above the knee, and being next covered with a dry flannel, should be kept *in situ* for six consecutive hours, after which it ought to be removed. Next, let the whole leg and thigh be gently rubbed with a hand dipped in a warm mixture of olive oil and laudanum, for many minutes; using, all the while, a gentle force of pressure in the bathing, which being completed, every particle of the oil should be carefully wiped away with a soft napkin, and the limb allowed to rest, after being wrapped up in fine warm flannel for the next four hours. These four hours being elapsed, the acid fomentation is repeated for another six hours, to be followed by the opiated-oil frictions; and so on, alternating the treatment.

669. There are very few examples of milk-leg that remain unresolved for more than forty-eight hours, when treated as I have above advised. I have pursued the plan for many years, and I believe I am indebted for it to consultation I had with the late Professor Dewees, many years ago.

670. I must now bring to a conclusion this work which I so long ago undertook to produce, in conformity with an engagement entered into while bidding you all farewell in our Class-room. I know full well that you have been disappointed at the delay in thus filling up a void in my course of winter Lectures, but I have to hope that you will accept my excuses for it, seeing what very constant and fatiguing professional avocations perpetually intervened; so that, after vainly waiting for and expecting some interval of leisure that I might properly devote to my task, I was compelled to commence and finish these Letters just as I send them to you now. From what I have written in them, it is easy to perceive how impossible a thing it must be for a Professor, in an American College of Medicine, to enter into a full discussion of childbed fever; and that whosoever he may be that gives a course of lectures here, he must be content merely to skim the surface of this important subject, and present, at most, only very general views of it. An American Professor of Obstetrics has too many other things to do, to rest long enough among these great considerations. Indispensable details in regard to the special anatomy and physiology of the woman; her diseases and accidents, whether as single or married; the informations required on the subject of embryogeny; labors; the lying-in state, and the cares

due to the new-born child, fill up to overflowing all the hours of a winter curriculum. You will please to remember that I spoke to you only two days on the subject with which I have here occupied 356 pages.

Look at my section 481, to see what Dr. Hulme says concerning his own feelings whenever he suspected patients of his to be even remotely threatened with an attack of childbed fever; or, if haply, since our last parting, you have yourselves been compelled to strive with this deadly enemy of women, remember the keen and searching anxiety with which your whole ministry in the cases was fulfilled; and I believe you will be ready to say that we all want more information on the subject than we have as yet obtained. I confess that, for myself, I should greatly rejoice could my own knowledge of it become far more copious and precise than this book shows it to be.

671. It is not for me, therefore, to judge whether, in fulfilling my promise to the Class, I have executed the task in such a manner as to meet and satisfy your reasonable expectations. Yet, while I freely leave you to judge in this matter, I beg to claim at your hands some kind consideration of the pains I have taken to render these Letters as explanatory and instructive as I could. To do this in a right way, an author ought to enjoy some leisure, and be, at least, free from the constant interruptions and fatigues, and carking cares of a practitioner's life. There should be time allowed him to collect and collate the numerous authorities that he might desire to cite in the course of his expositions or arguments. When such leisure is not attainable, one must either consent to hold his peace, or to write hurriedly. In the present case, I fear you may find reasons to suppose that I have had many professional duties to perform, that have conflicted with my wishes to make these Letters more worthy to be presented to you than they are.

672. You, my dear young gentlemen, have been so uniformly indulgent to my many faults as your teacher, that I cannot but look, with a feeling of confidence, for a continuation of that kindness, in at least, a friendly reception of these Letters of mine. I hope you may find in them some good and useful hints as to practice, if not also take out of them some fruitful suggestions, that may lead to thoughts beneficial on collateral subjects.

673. Let me earnestly beseech you to consider your ways, in the Medical life, and to take daily, higher and still higher views of that mission in society with which you are invested. I would it might please the Divine Author of the Universe to fill your minds with wisdom, and your hearts with philanthropy, and so, make you become worthy members of the great class of the physicians of America. May you never forget that you have a country to which you owe allegiance, not as its defender and taxpayer only, but as one of its Scholars, called on to contribute to its glory and honor, by helping your whole class to become more learned, more pure, more useful. The glory and renown of our native land will hinge not so much upon her numerous and growing population, her wealth, and her power on land and at sea, as in her educational pre-eminence, with the advances of which she continually rises in the estimation of mankind, and which alone can cause her to live in future story. Never then, for a moment, forget that your country expects from the hands of her Scholar-classes all that they can do to assist in elevating the standard of learning among our people, and that much is to be looked for from American physicians. Whose is the name, among all of you—that shall hereafter be pronounced by distant ages, along with the names of Hippocrates, or Sydenham, or Schwann? There is not a man of you incapable of either helping or hindering the great work of progress for our common calling, and so, in augmenting the boundaries of science and the arts that wait upon it, in all the various departments of knowledge. Be not then hinderers, but, what is far better, be ye true helpers in this great thing.

From your grateful friend and servant,

CHARLES D. MEIGS,

324 Walnut St., October, 1854.

TO THE STUDENTS OF MY CLASS OF 1850-51.

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